

RESPECTIVE EFFECTIVENESS OF SUCCESS
AND TASK-TENSION IN LEARNING¹BY J. NUTTIN, *Psychological Laboratory of Louvain University*

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I. INTRODUCTION

In regard to the mechanism of behaviour, two main problems present themselves: first, how the individual *acquires* the behaviour patterns he has at his disposal; secondly, how these behaviour patterns are put into action in actual life.

The first question may be reduced, in part, to that of learning and the evolution of behaviour; the second is that of performance and motivation.

This paper deals with the first problem only and presents certain relevant new experimental data.

All the researches of the past few years concerning the Law of Effect, and recent studies on conditioning, bring out the importance of failure and success in almost all learning processes. But agreement is less when it comes to answering the question *how* failure or success (i.e. how the result of a reaction) influences further behaviour.

It is Prof. Thorndike's opinion, as is well known, that success—or what he calls *reward*—*directly* strengthens the connexion between a given situation and the response which has led to success. It would follow that in virtue of this reinforced connexion behaviour, later on, leads *automatically* towards the successful reaction. Thus the direction to success or the 'finality' in behaviour, would be explained by the mechanical influence of the result on the connexions in the nervous system. This theory is based on the fact that in some types of experiments on learning, rewarded responses are, *unconsciously* and *automatically*, more frequently *repeated* than punished ones.²

II. SUCCESS AS SUCH AND TASK-TENSION

The first question we have examined concerning this important theory is whether it is indeed the *success as such* that strengthens the rewarded connexions.

¹ In this article the author intends to give only a *general* account of the main results of his experimental and theoretical work concerning the Law of Effect, done from 1938 to 1947 in the Psychological Laboratory of Louvain University (Belgium). The experiments here referred to will be described in more detail in a book to be published next year. Other experiments on unconscious learning and on the phenomenon of 'Spread' are not mentioned in this article. Their results confirm the conclusions reached here.

This paper was read at the annual meeting of the British Psychological Society at Dartford, April 1947.

² In Thorndike's conception, *learning*, or strength of connexions, is identified with *performance*. It is not our purpose to discuss this problem. But we have examined experimentally the question whether the rewarded responses are only, as a matter of fact, more frequently *repeated* (performance), or whether they are also more strongly *fixed* than the punished ones, in the sense that the subject *is not able to reproduce* the punished responses as exactly as the rewarded ones.

Our experimental results show that in serial learning the rewarded responses are actually better *learned*. Therefore, the Law of Effect seems not to be a law of performance only.

Prof. Thorndike himself tells us, in the descriptive parts of his books, that several factors—as, for instance, the total attitude of the subject—can play a part in the learned behaviour; but in the interpretation of the experimental results of his work, he takes into account only the factors of success (reward) and failure or punishment.

These factors, however, are not the only ones at work in his experiments. There is, in the attitude, or total set, of a subject, in an experiment on learning, the fact that he finds himself confronted by a *task* to be accomplished. This task *is not completely performed* by the simple fact of the subject's responding to each item presented to him. In fact, the *same* series of items has to be repeated, and the task of the subject consists in giving the highest possible number of successful responses in the subsequent repetitions of the series.

The experiment puts the subject into a state of *persisting* 'task-tension' in regard to each item of the series, and persisting task-tension may be a factor which, eventually, plays a part in the fixation or learning of some reaction patterns.

III. EXPERIMENTS WITH REWARD ONLY

The method of our experiments consists in separating the reward factor from the task-tension factor, to see what is due to both.

To do this we have imagined situations in which the subject receives a reward, or a punishment, for each response given, *but without his suspecting in any way that a further task will have to be accomplished concerning the same items*. For example, he is presented successively with forty cards. On the first card may be represented a group of people; on the second a flock of sheep, on another an avenue of trees or a street with houses, and so on. The subject is given the following instruction:

"I am going to show you, one after the other, a series of cards like this one [a card outside the series is shown]. I want you to estimate, by one glance at the card, the approximate number of objects you see on it. The number you give must always be a multiple of 5 (for instance: 35 or 40, and not 37). I want to find out by this experiment if you are able to make an approximate idea of the number of objects you see in front of you. You will have two seconds only for each card, so you will not be able to count the objects. After your estimate of each card, I will say to you 'Right' or 'Wrong' according as your evaluation was a good or a bad one."

In these experiments the subject thinks that his task with each card is finished when he has made a single response. This, then, is the first difference between our experiments and those of Prof. Thorndike: namely, there is no *persisting* task-tension attached to each card-situation. But the factors of reward and punishment are present, none the less, and even in a greater degree than in most of the classical experiments; for here the work of the subject is not merely to guess, but to make an estimate which can be definitely and objectively assessed.¹ The method should enable us to determine the influence of reward *in the absence* of a persisting task-tension.

There is another difference between our method and that of Prof. Thorndike.

Thorndike expressed the 'strength' of the connexions learned in terms of the percentage of repetitions of the same responses during the learning. But experimental data of Tolman and others concerning 'latent learning' have shown, that certain connexions may exist or

¹ It should be noted that success, in such experiments (i.e. the experimenter's 'Right'), has a real and considerable reward value for the subject. Failure, on the contrary, is really, and sometimes visibly, annoying.

be set up without revealing their presence in overt behaviour. This depends, as a matter of fact, on the *motivation* of the subject.

Now, in experiments on learning the motivation of the subject is not affected in the same way by good and bad responses. The resulting difference in motivation-attitude can eventually influence the frequency of *repetitions* of responses of both types.

We therefore tried to put our subjects, after they had once dealt with the cards, *in the same state of motivation for both the good and bad responses*.

New instructions were given to the subject when he had completed his estimates in regard to all the cards. He was then told simply to reproduce, for each card presented again, the number he had given previously.¹

Afterwards it was ascertained if he had reproduced more exactly the responses rewarded than those for which punishment had been given.

IV. EXPERIMENTAL RESULTS

In all our types of experiments, made on a great number and variety of subjects, we find that the subject is unable to reproduce the rewarded responses more exactly than the others. The average percentage of the exact reproductions of rewarded responses is not different from that of the punished ones. Some experiments show a slight and non-significant majority of rewarded responses exactly reproduced; others an equally non-significant majority of punished ones.

For instance, the total result of a series of experiments on 280 subjects yields 48.4% exactly reproduced responses for rewarded, and 51.6 % for punished responses. (In all our experiments of this type, there was an equal number of rewarded and punished responses.)

The individual results are indicated in Fig. 1. In the centre of the abscissa we have the subjects who *reproduced exactly* an equal number of punished and rewarded responses. The second step indicates, on the right, the subjects with *one* rewarded response *in excess* of the punished; and on the left, the subjects with one punished response in excess of the rewarded; and so on, for the various other steps of the abscissa.

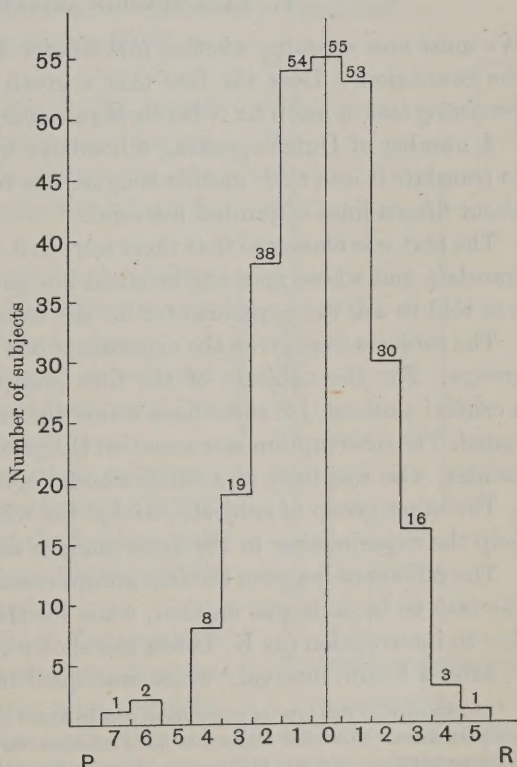


Fig. 1. Influence of reward on the strength of connexions.

P = number of punished responses *in excess* of the rewarded ones.

R = number of rewarded responses *in excess* of the punished ones.

¹ The subject is not *shown* the cards this time. The experimenter only names the cards, indicating the objects represented on them. Thus the subject is not tempted to make a new estimate instead of simply trying to reproduce the number given before.

It is clear from the graph that an almost equal number of subjects give a majority of exact reproductions in the two senses. And the differences in the relative number of rewarded and punished responses exactly reproduced, spread according to the normal curve, as is shown on the graph.

This means that the connexion between the stimulus and the response *rewarded* is not more strongly formed than the others.

In this experiment, reward and punishment were only applied *once* to the same connexion. In other types of experiments on unconscious learning, however, we have applied a reward as many as twelve times to the same connexion and a punishment twelve times to other connexions during twelve successive presentations of the same series. The results are just the same.¹

We can conclude, therefore, that success or reward *as such* is not the factor responsible for making the good responses more strongly fixed.²

V. TASK-TENSION AND STRENGTH OF CONNEXIONS

We must now examine whether *task-tension as such* has any influence on the strength of the connexions. Does the fact that a given *S-R* connexion forms part of a system of *persisting* task-tension fix it better than a connexion which is not part of a persisting task?

A number of Dutch-speaking schoolboys were given a text in French. They were told to translate it into their mother tongue. The text consisted of an interesting short story of about fifteen lines of printed material.

The text was chosen so that there were in it five of six words which the subject could not translate and whose meaning he could not guess from the context. In the instruction he was told to ask the experimenter for the translation of the words he did not know.

The subjects were given the experiment one by one, and were divided into two equivalent groups. For the subjects of the first group the translation work was interrupted at a critical moment, i.e. three lines before the end, so that they did not know how the story ended. The interruption was made on the pretext that the subject had to help the experimenter, who was busy at a small manual operation at a nearby table.

The other group of subjects *finished* the whole translation and was *afterwards* asked to help the experimenter in the same manner as with the first set.

The difference between the two groups consisted solely in this, that for the second group the task to be done was finished, while for the others there was a persisting task-tension due to interruption (as K. Lewin has shown).

After a 5-min. interval,³ which was spent in manual work with the experimenter, each

¹ An account of this type of experiment can be found in the author's article "La loi de l'effet et la finalité du comportement. Nouvelles recherches sur l'influence du succès", published in *Miscellanea Psychologica Albert Michotte* 1947, pp. 611-32. Editions de l'Institut Supérieur de Philosophie, Louvain.

² This conclusion was first reached after another group of experiments, carried out in 1939 and published in polycopy during the war (1941). Cf. J. Nuttin, *De Wet van het Effect en de Rol van de Taak in het Leerproces*, Louvain, 1941. And also, from the same, "De Finaliteit in het menselijk handelen en het Connectionnisme", in *Tijdschrift voor Philosophie*, 1942, iv, 235-68. When, in 1946, the American reviews became available to us again, we were pleased to discover that Wallach and Henle have reached results which confirm our conclusion. Cf. *J. Exp. Psychol.* 1941, xxviii, 340-49; 1942, xxx, 147-60.

³ For the group that continued to the end, the 5 min. were counted from the moment they reached the point where the subjects of the other group were interrupted. But let us point out that the group of subjects which translated right to the end worked only a few seconds longer than the others, since the translation of the last three lines presented no further difficulty.

subject was required to translate a few words. The words put to him were the ones he could not translate a little while before and whose meaning the experimenter had told him.

The aim was to discover whether the connexion between the foreign word and its translation was better formed in the case of the group which was not allowed to pursue its task to the end.

Obviously no task-tension was attached to the pairs of words themselves. (This is a difference between our experiments and those of Zeigarnik.) The subject, in fact, did not need the paired words in order to finish his job. The only difference lay in the fact that for the group with the unfinished task, the connexions were an *integral part of a whole task* not in fact as yet completed.

VI. RESULTS

The results of this experiment were decisive. The thirty-six subjects whose task was interrupted, gave, on an average, the exact translation for 76 % of the words put to them, and the other group for only 46 %. This difference is statistically significant.

The median result for the interrupted group was 80 %, as compared with 40 % for the other group.

The individual results are represented in Fig. 2. The dotted line registers the results of the finished-task group; the unbroken line those of the interrupted-task group. On the abscissa is indicated the percentage of translations of words exactly reproduced.

The first step indicates the subjects who have exactly reproduced from 0 to 20 % of the words asked. There are six subjects of the finished-task group in this lowest class.

The second step indicates the subjects who reproduced from 21 to 40 %. The majority of the finished-task group lies here, and only three subjects of the interrupted group.

On the contrary, the majority of the interrupted-task group is situated in the highest step with a percentage of 81-100. So we have an accumulation of subjects of the finished-task group in the lower percentages and an accumulation of the interrupted-task group in the higher percentages.

We conclude from this experiment that the *S-R* connexions embodied in a system of persisting task-tension are, *by virtue of this very fact and without any memorizing*, more strongly established for the time that the task-tension lasts. (Experiments concerning this last question were also carried out.)

It is clear, therefore, from these two series of experiments, *that the strengthening of the connexions is in relation, not to the reward or success as such, but to the persisting task-tension.*

(This factor of persisting task-tension, be it noted, is present also in the Thorndike experiments.)

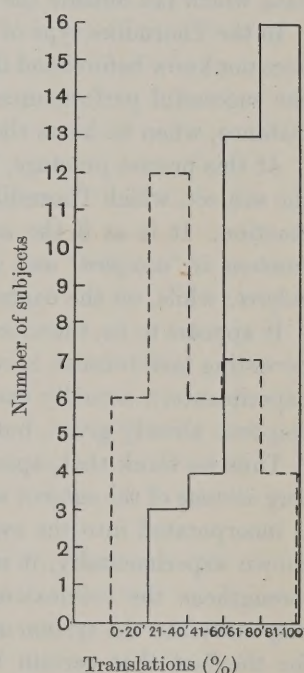


Fig. 2. Influence of persisting task-tension on the strength of connexions.

— = Interrupted-task group.
 = Finished-task group.

VII. THE ROLE OF SUCCESS OR REWARD

How does it come about that rewarded or successful responses are incorporated into a system of task-tension? What is the *role* of reward in this incorporation?

With Prof. Thorndike, we reject, as being an unsatisfactory explanation in a great number of experiments, the conscious memorizing of the right responses. In many experiments reward cannot possibly direct an effort to memorize on to certain responses.

But in experiments with persisting task-tension, the words 'Right' or 'Wrong' pronounced by the experimenter—or to put it more generally success and failure—have a *twofold significance*.

In point of fact, we know that an individual with a task to perform, or a need to satisfy, is really in a *selective attitude* with regard to the data that present themselves to him. His attitude towards material which is experienced as a direct means to the accomplishment of his task, or the satisfaction of his need, is not the same as the attitude he adopts to data which fall outside the sphere of his present active orientation.

In the Thorndike type of experiment, as well as in many real life situations, the subject does not know beforehand if the reaction or response he achieves is really a good means for the successful performance of his task. It is only when the result is experienced—for instance, when he hears the experimenter say 'Right' or 'Wrong'—that he is sure.

At this precise juncture, when the result becomes known, a new reaction is aroused in the subject, which Thorndike himself did not fail to notice, and which he calls the 'O.K.' reaction. It is as if the subject said to himself: "Yes, that's it." Thus *the successful reaction is 'accepted' and is incorporated (or integrated) into the selective attitude of the subject*; while, on the contrary, *the punished response is 'let go' and abandoned*.¹

It appears to us, therefore, that, in the psychological situation of a subject in a state of persisting task-tension, success or failure (e.g. the 'Right' or 'Wrong' pronounced by the experimenter), actually exercises a twofold function. 'Right' signifies a reward for the response already given, but it also *directs the selective attitude of the subject*.

Thus we think that, apart from any intentional or conscious effort, but *in virtue of the very attitude* of the subject who has a task to do or a need to satisfy, the rewarded response is incorporated into the system of active orientation or task-tension. And, as we have shown experimentally, it is the fact of being fitted into such a system of tension that strengthens the connexions. In other words, it is the selective fitting of successful responses into the system of tension created by a persisting task or need, which accounts for the fact that certain behaviour patterns are more solidly fixed and kept by the organism.²

¹ Concerning the directive and informative value of punishment as compared to reward, in serial learning, see also A. Dand, "Reward and Punishment in Learning", *Brit. J. Psychol.*, xxxvi, 1946, 83-7.

² The author does not exclude a more conscious informative function, exercised by reward and punishment on the higher levels of our activity. But he thinks that, beneath all conscious effort and information, the described process of automatic integration in a tension system is a fundamental one in the fixation of behaviour patterns. In acquiring motor reactions it may be eventually the only effective mechanism. In another experiment, *selective integration of some items out of a series of S-R connexions* was also obtained.

VIII. CONCLUSIONS

1. The fact that behaviour develops spontaneously towards successful reactions is linked up with the fact that these reactions are better preserved by the organism. Success as such, however, is not the responsible factor in this stronger fixation; but the selective integration of successful reactions into the tension system created by our needs and tendencies.

2. The process of incorporating successful responses into the active orientation of the subject is one of the fundamental processes in the acquisition of new behaviour patterns.

3. It is impossible to explain the direction or the 'finality' of behaviour by the mechanical and retroactive influence of success, as Prof. Thorndike thinks. On the contrary, we have shown that an active direction or final orientation of the subject must be *presupposed* if we are to explain the stronger fixation of successful reactions.

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THEORY OF THE HUMAN OPERATOR IN CONTROL SYSTEMS¹

I. THE OPERATOR AS AN ENGINEERING SYSTEM

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I. The human operator behaves basically as an intermittent correction servo (p. 56). II. The intermittent corrections consist of 'ballistic' movements (pp. 56-58). III. There are some counteracting processes tending to make controls seem continuous (pp. 58-59). IV. Electrical models could fairly exactly simulate the human operator's behaviour in tracking (pp. 59-61).

I. *The human operator behaves basically as an intermittent correction servo.* The evidence for this is the periodic or 'wavy' nature of the time-record of tracking errors, showing a spectrum with a predominant frequency of about 0.5 sec. with a smaller cluster of frequencies from 0.25 to 1 sec. This periodicity might be attributed to a sensory threshold or 'dead zone', such that misalignments smaller than a certain value evoke no corrective movement; but there is evidence against this. First, the display-magnification is usually such that the misalignments occurring during steady tracking exceed the known threshold (i.e. visual acuity). Secondly, if the rate of the course, or the magnification of the display for a given course, is increased by a certain factor, the periodicity of the corrections is little, if at all, affected; whereas if their periodicity were determined by the time taken for the misalignments to reach a certain 'threshold' value this alteration should shorten the periodicity of the corrections in the same ratio.

Consistently with the above principle, we find that the mean error in tracking any given variable-direction course is nearly proportional to the rate of the course, over a wide range of speeds. We should account for this by saying that the faster the course the greater the misalignments that occur in each period between two corrections, in strict proportion.

II. *The intermittent corrections consist of 'ballistic' movements.* For example, they have a predetermined time-pattern and are 'triggered off' as a whole. This behaviour may be contrasted with that of an intermittent correction servo in which, for instance, a follow-up motor is intermittently switched into a circuit in which it runs until it has reduced the misalignment to zero, and this action reduces the input to it to zero so that the motor stops. In the human operator, on the other hand, at a particular instant (i.e. about 0.3 sec.

¹ *Editor's note.* The material of this paper and of a second one to be published in the next number of this *Journal*, was prepared by the late Dr Craik in March and April 1945. It was written substantially in the form in which it now appears, and undoubtedly was intended, not as a final form for publication, but as a preliminary draft for discussion. In view, however, of the very lively and widespread interest that has developed in the problems discussed in these two papers, and of the attractive and original way in which Craik handles them, it seems highly desirable that they should be made public. The present paper deals with the possibility and significance of the construction of mechanical and electrical models for simulating human behaviour in any kind of continued pursuit task. The second paper deals with fundamental physiological and psychological mechanisms which appear to be involved in operations of the same kind.

Most of the work of preparing the articles for publication was done by Miss M. A. Vince.

after the end of the preceding corrective movement), a corrective movement having a predetermined time-course (usually occupying about 0.2 sec.) is triggered off. The evidence for this is based on studies of reaction time, i.e. on the internal time-lag of the operator, due to the time taken by the sense-organ to respond, for the nerve-impulses to traverse the central nervous system, for the appropriate response to be 'selected' and for the nerve impulses reaching it to traverse the motor nerves. This lag is about 0.2–0.3 sec. Thus, if a human operator's limb movement amplitude, or velocity, or acceleration, were determined continuously by the misalignment, continued oscillations of approximately 0.5 sec. period, and of whatever amplitude they commenced at, would inevitably result. This tendency could be overcome if a misalignment triggered off a ballistic movement of fairly correct amplitude (say $\pm 10\%$); the eye would then detect the residual misalignment, which may be composed of two parts—the error in the first ballistic movement, and any movement of the target which has occurred in the meantime. In the absence of the latter (e.g. in aiming at a stationary target) the second corrective movement may again be accurate to $\pm 10\%$ of its own value so that the misalignment is reduced to 1 % of its original value.

Direct evidence bearing on this ballistic behaviour can be obtained in various ways. First it is easy to present a misalignment to an operator, and then screen his eyes just before he makes his corrective movement. A movement accurate to within about 10 % will result, if he has previously learned the 'feel' of the control. If he operated like a follower motor, intermittently switched in, such obscuration of the misalignment would be equivalent to cutting the input connexions, and the motor would not of course make any further corrective movement.

Physiologists might make a further hypothesis to avoid the 'ballistic' theory. They might say that the visual misalignment, once it has been detected by the eye, becomes translated into a 'limb-movement-misalignment' (i.e. a kinaesthetic misalignment) which acts as the continuous input to the limb until our kinaesthetic (i.e. joint and muscle senses) register 'correct position', or no misalignment. We certainly do possess such a sense, but it is less easy to abolish it experimentally and to see what happens in its absence, than in the case of vision. Patients with *tabes dorsalis* have considerable loss of kinaesthetic sensation, but it is difficult to know how complete this loss is in any particular case.

In any case, the same general argument as before—that a continuous series of oscillations of the initial amplitude would occur if elimination of kinaesthetic misalignment were the sole determinants of movements, owing to the inevitable reaction-time lag—seems to apply. Further, it is possible to show that considerable precision of movement is maintained even when the movements are made so rapidly that they are completed before the kinaesthetic stimulus corresponding to their first approximation to the right position could have 'gone the round' of the central nervous system and controlled the subsequent output-movement; unless indeed reaction-times to kinaesthetic stimuli were vastly shorter than to any other kind. It is, however, possible to show experimentally that kinaesthetic reaction times are very little shorter than auditory, for instance. Thus, we may ask the operator to move a lever against a stiff spring, so as to make a rapid movement to correct a misalignment, and then to return to his starting point. After he has learned the 'feel' of the control (i.e. its gear ratio and spring tension) and is making fairly accurate movements, we suddenly alter the spring tension, so that he over- or under-shoots. A record

of this on a fast-moving drum shows that about 0.15 sec. elapses after he begins his movement, until he is able to begin a readjustment of it, to meet the modification of resistance.

Similarly, it is possible to show that in playing musical instruments, typewriting, sending morse, etc., complicated patterns of movement are executed at a rate which would be impossible if they were continuously governed by the value of the misalignment, with the inevitable reaction-time lag. Apparently they must be individually performed triggered off ballistically, and the sensory feed-back must take the form of a delayed modification of the amplitude of subsequent movements. Sensory control, in other words, alters the 'internal gear ratio' or amplification of the operator with a time lag and determines whether subsequent corrective movements will be made; it does not govern the amplitude of each individual movement while it is being made. We could make a servo, using existing engineering principles, which would show the features of intermittent, ballistic correction. But this last point—the fact that the sensory misalignment alters not merely the amplitude of the response but the *relation* between the input, or misalignment and the output, or response—introduces a further complication, the nearest approach to which, in engineering, seems to be 'floating plus proportional control'. Even this involves a quantitative alteration of the amplification of the system by the residual misalignment, whereas something more complex still seems to be occurring—a very wide alteration in the functional relationship between input and output.

For instance, if the operator is using a positional control, his successive ballistic corrective movements should be linearly proportional to the misalignments; but if he is using a velocity control they would have to be linearly proportional to the *derivatives* of the misalignments. Roughly speaking, we might call this *qualitative* modification or output on a basis of some response to the difference between instantaneous input and output at the previous instant, or 'qualitative feed-back'.

A further complication is introduced by the operator's ability to 'anticipate' movements of the target, or alterations in misalignment. For instance, with a positional control, the errors in tracking a moving target are usually less than we should predict on the above theory of intermittent ballistic corrections. The operator goes on turning the handle steadily, or even accelerating it; his record, after some practice, becomes much smoother than it was initially; and if he finds that he is still lagging behind the target (as if the above theory is correct he is bound to do) he can put in an extra forward movement. Here we have several processes.

III. *There are some counteracting processes tending to make controls seem continuous.* First, there is one akin to momentum, or inertia. If the operator has been turning the handle, in a series of discrete movements, for some seconds, he will tend to convert this into as steady a rotation as he is capable of, and to continue doing so although the misalignment may be zero, i.e. he has zero input to produce this output! (This can be shown experimentally by suddenly stopping the target, when the operator will overshoot for the period of one reaction time, until the serious misalignment which results stops his steady output from continuing.) It is for this reason that in Section I it was stated that the human operator is *basically* an intermittent correction servo; he has in addition this mechanism for going on doing whatever is giving a satisfactory result, or zero misalignment, rather like a heavy flywheel, and having the same valuable smoothing effect.

What are the essential features of this process, and can we conceive any mechanisms

which will accomplish it? When the operator continues to turn the handle at the same speed, independently of whether there is any input or not, he is, in humanistic terms, assuming that he is justified in doing so, in order to compensate for his reaction time lag. Since he is always subject to this lag, in attempting to keep up with the present he is always in fact being a prophet and extrapolating from past data! It is really no different from the further kind of anticipation which enables him to extrapolate into the physical future. Now all scientific prediction consists in discovering, in data of the distant past and of the immediate past which we incorrectly call the present, laws or formulae which apply also to the future, so that if we act in accordance with those laws our behaviour will be appropriate to that future when it becomes the present. Thus the essential feature of extrapolation and anticipation is, again in humanistic terms, that the operator should detect the *constants* in what he is doing. Thus, he may move a handwheel in a series of jerks, so that its *position* changes from moment to moment, but he may realize after a few seconds that he is turning it at a *steady rate*, i.e. its *angular velocity* is constant; and having discovered this he may try whether it will not pay him to go on doing so; usually it will. He may, however, find that its rate is changing—the target has angular acceleration. He may, in theory, at any rate, be able to feel this acceleration which he is having to put into the handwheel, and if he happens to find that it is constant, or nearly so, and is able to put out a constant acceleration of this value in turning the wheel, again he may achieve better following.

Now let us look at the same thing from a mechanical point of view. There are many devices—such as speedometers and accelerometers—which do the differentiations involved in recording velocities and accelerations; and the problem would be how, for instance, to couple a number of such devices to a telephone selector-switch operating motor, so that if the output of the motor over a few seconds of intermittent corrective action showed a constant reading on the speedometer, or even on the accelerometer, the motor would be caused to go on putting out this speed or acceleration, irrespective of whether there was any input or not, unless or until such behaviour gave rise to a large misalignment. If that happened the extrapolating system would be overridden and intermittent corrections would begin again, until a new value for a constant was found. The solution would seem to be to provide the motor with positive feed-back of such a kind that it continued to go on doing whatever it was doing at the moment—running steadily or accelerating uniformly. Such a system would need considerable smoothing and stabilization, otherwise any slight disturbance, such as a slight acceleration, would very rapidly be cumulative, and the machine would reach its maximum speed; but if the feed-back were delayed and smoothed, the system could be sufficiently stable and would not ‘wander’ too badly. This system would of course be combined with negative feed-back of the ordinary kind (viz. actuation by the difference between input and output quantities), so that if the positive feed-back led to overestimation of the velocity, or if the target started to decelerate, the motor would overshoot and this would introduce a positional misalignment, which would reverse the direction of mechanical control. This would alter the average value, for the last time-interval, to the positive feed-back system, which would therefore cease from perpetuating this velocity but would, when it had time to steady down, start putting in a new one.

IV. *Electrical models could fairly exactly simulate the human operator's behaviour in tracking.* In general terms, the extraction of the inputs for the positive feed-back network consists of successive differentiations, while the extrapolations on the basis of them consist

of successive integrations. Let us consider in more detail some circuits by which this might be accomplished. Suppose the motor drives a generator across whose output terminals is a capacity in series with a high resistance, constituting a differentiating circuit with a time-lag or averaging effect, owing to the time-constant of the system. Then the generator voltage is proportional to the speed of the motor and the voltage across the resistance of the differentiating system is proportional to its acceleration; if necessary, higher derivatives can be obtained in the same way. The lag in the first differentiation can be obtained by putting a resistance and capacity in series across the generator output and taking the voltage off the capacity. The output voltage from this smoothing system is taken to the input of the amplifier supplying the motor fields, and should cause the motor to continue running at the mean speed at which it has been manually rotated for a sufficient time to cause the voltage delayed and smoothed across the generator to reach a steady value. The speed of running will of course wander slowly in time if the system also has ordinary velodyne negative feed-back for velocity control. If the manually applied speed was an accelerating one, the system will maintain a mean steady speed if it is supplied with one differentiating stage only (i.e. the generator with its delaying system). But if there is a second differentiating system, with a longer time-constant, it will register a manually imposed change of velocity over several periods of operation of the first differentiating system, i.e. an acceleration, and if this is integrated by a resistance-capacity circuit and applied to the amplifier serving the fields, a uniform acceleration will occur.

Of course, it is not necessary for the original speeds to be put in manually; with a velodyne fitted up as a servo auto-following system in which the task of the velodyne is to make a slider keep on the centre of a potentiometer, for instance, which is moved by an external agency, the mechanical control will commence by ordinary positional following, being actuated by the misalignments. Further, though this alone would lead to a lag behind a uniformly moving target, once it has started to run at a constant velocity, the remaining misalignment will still be operative, if arranged to be in series with the positive feed-back voltage, so as to cause the shaft to step on and make up for the lag. This system would have many of the same characteristics as a velodyne with phase advance produced by delayed negative feed-back—i.e. a condenser across the generator output.

It should be possible to make a velodyne simulate the 'intermittent ballistic correction' process considered in principles (I) and (II). Thus, the error-voltage representing the misalignment could be connected periodically by a rotating contact to a condenser which is charged. This condenser would then be switched on to the amplifier input and would result in a 'ballistic' rotation of the output shaft through an angle proportional to the charge on the condenser.

Little is known of possible physiological mechanisms for accomplishing this kind of thing. There is evidence (e.g. from sensory adaptation and accommodation of nerve) of differentiating systems, at least of the first order, which may serve to measure rates of change of stimulation, though our knowledge extends only to stimulus *intensity* and not to more complex stimuli such as misalignments in space. Even here, it is possible to suggest hypothetical spatial differentiating systems which are not physiologically inconceivable. The other aspect—the integration, resulting from positive feed-back—would seem to require 'autorhythmic' nervous centres which continue to discharge once they have been forced to do so, and in a way which follows the original forcing stimulus. The beating of the isolated frog's heart and the spontaneous oscillatory potentials in the excised frog's brain

and in the intact cortex of man (both Berger rhythms and the abnormal rhythms of epilepsy) are suggestive in this respect, for they are evidence of self-maintaining neural oscillators. Lorente de No's and Ransom's concept of the 'closed neurone circuit' would serve the same purpose. What has to be considered is clearly a form of positive feed-back and the main difficulty in all the cases just mentioned would seem to be that what is required is continuous feed-back of excitation in the form of nerve-impulses following after the neurones have recovered from their refractory phase, whereas slow potential oscillations probably imply discharges of some other kind than trains of nerve impulses.

We should also consider long-lasting changes of stimulus-response relationship (i.e. learning) which, in an electrical model, would probably require to be imitated by some autoselective switching device rather than regarded as time-constants of a resistance-capacity system. Another type of control demands the establishing of complex response-patterns which are 'triggered off' as a whole by the stimulus. Instances are the action of word-habits in typewriting, or of blocks of stimuli in transmitting morse, or of associated movement groups in knitting. These seem to require some 'sequencing' switchgear, of the type used in the Relay Automatic telephone system, and make us think of the physiologists' 'chain reflexes' and of rhythmic reflexes such as walking and breathing.

(Manuscript received 4 March 1945)

NOTES ON THE INTELLECTUAL AND SOCIAL CAPACITIES OF A GROUP OF YOUNG DELINQUENTS

By M. I. DUNSDON

In November 1941 an analysis was made of cases referred by magistrates and probation officers to an L.E.A. child guidance clinic between 1939 and 1941. The publication of this analysis in *The Lancet*¹ aroused considerable interest. In view of the requests for further information of a similar kind, made by various bodies and individuals concerned with juvenile delinquents, the following brief communication concerning children and young persons who came before Juvenile Courts in the years 1941-5 may be of interest.

Until March 1944, the cases referred to the clinic continued to be those specifically selected by the magistrates and were therefore a 'selected population', whose intelligence distribution, educational attainments, etc., might have been suspected of presenting a rather different picture from that obtained from a more comprehensive sample of the total population appearing before the magistrates.

In 1944 it was decided therefore to assess the intellectual and educational abilities and to make a report on the personalities of all children and young persons who were brought before the Juvenile Court and remanded on bail or in custody. This, in effect, meant that from 1 May 1944 all children and young persons, excepting only those concerned in very minor offences for which they were cautioned and the charge dismissed, or were summarily fined, were seen and reported upon. This routine investigation represents probably the first attempt to report on the 'common population' appearing before a Juvenile Court, rather than on only selected cases.

Table 1 summarizes for comparison the distributions of intellectual ability found each year during the period 1939-45. It will be noted that the incidence of cases with normal and better ability has been summed for each year in the period 1939-43. It will be seen also that the differentiation into 'normal' and 'superior' groups from 1943 to 1945 (columns 4e and 5) does not give so detailed a grading of ability as is given in the three subnormal groups. Such grouping, however, served the purpose for which the table was originally prepared, which was to compare the incidence of cases average and above with those who ranked intellectually inferior, and more particularly to indicate the proportional incidence of cases falling within the three broad degrees of mental dullness.

In the period 1940-4 it will be seen that more than half of the total number of 'selected' cases fell into the subnormal categories. The subsequent extension of the investigation, in 1944-5, to include all cases on bail and remanded made, however, very little difference to the proportional incidence of subnormal cases found. This fact is brought out clearly in Table 2.

Comparison of the distribution of intelligence among the children and juveniles forming the total population appearing before a Juvenile Court during the period 1 May 1944 to 30 April 1945 with the distribution obtained from a cross-section of a general population of the same age range emphasizes still more clearly the abnormal distribution of the

¹ Juvenile Delinquency in Wartime. Bodman and Dunsdon, *The Lancet*, 8 November 1941, p. 572.

former. The markedly heavier incidence of mentally subnormal cases charged with offences may be realized by comparing the distributions found in columns 3 and 5 in Table 1, which are juxtaposed below, for easier comparison, in Table 3.

Table 1. *Distribution of intelligence of groups of children and young persons before a Juvenile Court*

1	2	3	4					5	
Intelligence expressed in ranges of I.Q.	Classification	Unselected population sample %	Selected cases referred to clinic by Juvenile Court					Unselected group: all cases remanded in custody and on bail 1944-5 %	
			(a)	(b)	(c)	(d)	(e)		
			1939-40 %	1940-1 %	1941-2 %	1942-3 %	1943-4 %		
115 and over	Superior	16.0	69.0	46.0	41.4	49.7	7.25	1.9	
85-114	Normal	68.0	84				35.5	40.2	
70-84	Very dull and border line	13.0		7.4	31.0	37.9	29.0	34.2	38.7
55-69	High grade feeble minded	2.0		22.1	21.0	15.9	17.9	15.8	16.6
Under 55	Mentally defective	1.0	1.5	2.0	4.8	3.4	7.25	2.6	
Total		100.0	100.0	100.0	100.0	100.0	100.0	100.0	
No. of children and young persons examined			68	100	145	144	152	313	

Table 2. *Percentage incidence of subnormal cases (I.Q. less than 70)*

Selected cases referred to clinic by Juvenile Court					Unselected. All cases
(a)	(b)	(c)	(d)	(e)	(f)
1939-40	1940-1	1941-2	1942-3	1943-4	1944-5
31.0	54.0	58.6	50.3	57.25	57.9

Table 3. *Comparison of intelligence distributions found in
(a) general and (b) delinquent groups*

Intelligence grades expressed in I.Q. ranges	(a)	(b)	Increment or decrement %
	Cross-section of general population	All cases on bail and in custody	
	%	1944-5 %	
115 and over	16.0	1.9	- 14.1
85-114	68.0	40.2	- 27.8
70-84	13.0	38.7	+ 25.7
55-69	2.0	16.6	+ 14.6
Under 55	1.0	2.6	- 1.6
Total	100.0	100.0	

Backwardness in school achievement is an all too common finding among delinquents, as might be expected from the incidence of subnormal intellectual capacity found among them. Even when those were set aside who, though mentally very dull, were achieving a level of school attainment in accordance with their innate ability, the incidence of those who failed to reach such a level was still significantly high. Furthermore, within this group, a notable proportion had not even attained a 'usable' degree of skill in either one or both of the basic subjects (reading and arithmetic), i.e. they had not reached an average 7-year level. Table 4 shows the proportion of delinquents so additionally retarded.

Among delinquent young persons of employable age, the incidence of educational retardation continues to be high, as Table 5 shows.

There were some young offenders who, in spite of a poor educational level, had shown practical initiative and exercised some degree of social responsibility, eventually becoming satisfactory workers. On the other hand, in the majority of those cases where, in addition to poor school achievement, there was often a longstanding record of unsatisfactory behaviour both at home and at school, subsequent work records almost invariably showed them to be unsatisfactory employees who did not settle in regular work.

When an assessment of social maturity¹ was made, it was found that where the social maturity ratio (social quotient) was poor, a 'lower than expectation' level of achievement at school and at work occurred almost without exception.

On the other hand, many young people of relatively poor intellectual endowment who, from force of circumstance, had been compelled to undertake social and domestic responsibilities beyond their natural scope, were found to have broken down and sought relief in a display of irresponsible behaviour not in accordance with their usual patterns of con-

Table 4. *Percentage of delinquents retarded educationally below innate intellectual capacity*

Reading	Arithmetic	Without a "usable" degree of skill in one or both subjects
35	50	25

Table 5. *Delinquent young persons, aged 14+ years, who were retarded educationally below mental capacity, allowance having been made for innate intellectual limitations*

Incidence percentage		Lacking a "usable" degree of skill in one or both subjects
Reading	Arithmetic	
50	65	22

formity. Typical instances of this happening were found among many girls who, besides being gainfully employed during the day, were expected to shoulder relatively heavy domestic responsibilities at home after work and consequently had little opportunity for the recreation which, as wage-earners, they felt to be their due. Such girls often were found to have left home on an impulse and subsequently either to have become involved in delinquencies which were less deliberate than incidental, or were found to have made undesirable associations which led them to appear before a court as 'in need of care and protection'.

In a similar way, many boys who, in the absence of their fathers, became the main, or sole, wage earners in their families, were unable to sustain for long the responsibilities attaching to such a role. Although, for a short time, the added prestige which they gained in the family and among their companions compensated for some of the disadvantages of their new position, they often, in order to maintain their newly acquired superior status, attempted to supplement their wages by dishonest means, or else tended to drift from job to job in a short-sighted attempt to avoid responsibility. This course eventually led many of them to resort to stealing to provide themselves with the money which they had lost through interrupted employment.

Though assessment of social maturity, on this particular scale, was not made as a routine measure until more than half way through the period 1944-5, a sample, fairly repre-

¹ Vineland Social Maturity Scale, revised.

sentative of the whole year group in such factors as age, intelligence, type of offence, etc., was so assessed. Table 6 gives the distribution of social maturity (expressed as a ratio of social to chronological age) in relation to intellectual maturity, the incidence being expressed as a percentage.

From these figures it will be seen, among other things, that although 59% of this group had below normal intellectual capacity (i.e. less than 85% of average), a still larger proportion (67.46%) showed a degree of social inadequacy (i.e. s.q. less than 85) which would be likely to hinder their adjustment to the demands of their social environment.

Another significant point brought out more clearly by this table is that there appears to be a remarkably high proportion of cases who, though broadly ranking as intellectually adequate (i.q. 85 and over), have attained a much less satisfactory level of social efficiency for their age. 18.07% of the sample assessed for social capacity had social maturity quotients below 85, in spite of having an i.q. above that level.

Table 6

Range of intelligence (I.Q.)	Social quotient range (s.q.). % incidence					Total
	Under 55	55-69	70-84	85-114	115 and over	
115 and over	—	—	—	2.41	—	2.41
85-114	—	1.20	16.87	20.48	—	38.55
70-49	—	8.44	19.27	8.45	—	36.16
55-86	—	4.82	12.05	1.20	—	18.07
Under 55	1.20	3.61	—	—	—	4.81
Total	1.20	18.07	48.19	32.54	—	100.00

Table 7

Chron. age (Years)	Social quotient incidence percentage of age group					Total % of age group
	Less than 55	55-69	70-84	85-114	115 and over	
16	4.80	19.00	19.00	57.20	—	100.0
15	—	20.00	46.70	33.30	—	100.0
14	—	21.43	57.14	21.43	—	100.0
13	—	12.50	75.00	12.50	—	100.0
12	—	40.00	40.00	20.00	—	100.0
11	—	16.70	50.00	33.30	—	100.0
10	—	12.50	75.00	12.50	—	100.0
9	—	—	50.00	50.00	—	100.0
8	—	—	100.00	—	—	100.0

In a distribution of social quotients within each age group, the relatively poor social development of the average delinquent is again stressed; it will be seen from Table 7 that, except in the 16-year old group, the majority of cases in each age group were socially backward. The slightly higher proportion of the 16-year old group who showed a more adequate social capacity may be a result of the fact that most of them had been employed and had at any rate attempted to cope with such responsibilities as their status as wage-earners imposed. Many cases had responded with reasonable success on the whole, though unable to maintain a constant standard of satisfactory behaviour. On the other hand, to offset this, a larger percentage of the relatively inadequate appear in this group, which is the only group to show an incidence in the 's.q. less than 55' range. This 4.8% of the group of 16-year olds, in other words, showed social foresight and competency of a level not above what would be expected from a normal child of 8½ years, so that it is hardly surprising that they found difficulty in meeting satisfactorily such social demands as could normally be met by young people of their years.

Though the relatively smaller samples in the lower age group ranges may account to some extent for the lack of spread in s.q. found for children in the 8- and 9-year old ranges, the same general tendency to social inadequacy and irresponsibility is found at all age levels.

Table 8 shows a similar distribution according to age and social quotient, but records the incidence within each category as a percentage of the total sample covered by all age groups.

Table 8

Chron. age (Years)	Social quotient incidence percentage of total sample					Total %
	Less than 55'	55-69	70-84	85-114	115 and over	
16	1.20	4.82	4.82	14.46	—	25.30
15	—	3.61	8.43	6.03	—	18.07
14	—	3.61	9.64	3.61	—	16.86
13	—	1.21	7.23	1.21	—	9.65
12	—	2.41	2.41	1.20	—	6.02
11	—	1.20	3.61	2.41	—	7.22
10	—	1.21	7.23	1.21	—	9.65
9	—	—	2.41	2.41	—	4.82
8	—	—	2.41	—	—	2.41
Total	1.20	18.07	48.19	32.54	—	100.00

The above findings, it will be seen, stress the importance of taking into consideration other factors, in addition to those concerned with intelligence alone, when dealing with the problem of delinquency. Still more is it necessary to give weight to the factor of social capacity in deciding upon preventive measures and the disposal of those who have been guilty of offences and have not responded satisfactorily to probation or other schemes intended to discourage recidivism. To what extent the making of such provisions should devolve upon educationists and how far they come within the province of other persons and bodies concerned with the social life and education of children outside school is a matter of controversy. Available evidence in any case indicates fairly clearly that those responsible for town planning and re-housing schemes should give serious consideration to the inevitable effects of a too high concentration of groups of families who show a marked tendency to what may be termed 'familial social incompetency'.

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THE GROWTH OF THE KNOWLEDGE OF TIME IN CHILDREN OF SCHOOL-AGE

By N. C. BRADLEY

I. *Introduction* (p. 67). II. *Method of experiment* (pp. 67-71): A. *Tests* (pp. 67-70); B. *Standard of marking* (pp. 70-71). III. *Results* (pp. 71-77): (1) *The 'Questions' test* (pp. 71-73); (2) *The 'Picture-Arrangement' test* (p. 73); (3) *The 'Order-of-Dates' test* (pp. 73-74); (4) *The 'Temporal-Absurdities' test* (pp. 75-77). IV. *Summary* (p. 77). *References* (p. 78).

I. INTRODUCTION

Psychologists have given little attention to the growth of the child's understanding of time as compared with the study of general intelligence, the special abilities or the special senses. Its Kantian companion, the category of space, has been a more attractive field for research, probably on account of its basis in the special senses, which have offered a directly ascertainable and objectively measurable range of responses. On the contrary, the perception of time is not the special function of any observable sense-receptor, and although there are available the results of many experiments on time perception, very few experimental studies have been made of the concept of time. This despite the obvious relevance of any experimental findings, when considered from a utilitarian standpoint, to the theory of the teaching of history (6, 9).

Apart from an investigation carried out by Sturt⁽²¹⁾ and Oakden & Sturt⁽¹⁶⁾ the only references to the problem are to be found in general works on child psychology and on intelligence testing (1, 3, 4, 5, 11, 15, 22, 23), where it is dealt with incidentally, and in the reports made by several observers of the development of individual children of pre-school age (7, 8, 10, 12, 13, 14, 17, 18, 19, 20, 21).

The present investigation was limited to school children of 5 years and upwards and here the aims were to trace the growth of ability to understand the ordinary time-words used in everyday life and the development of the conception of an universal continuous time-scheme extending into the past and the future. The tests used were mainly verbal, based on the assumption that from his use of words the child's concepts can be inferred. While this assumption is unwarranted when dealing with pre-school children, the dangers of misinterpretation are not so great among children of later age-groups, when greater facility with language has been acquired.

It will be seen that this investigation is partially a reapplication of some of the tests used by Sturt and Oakden, to whom the writer is deeply indebted. Some of their conclusions seemed unwarranted and unexpected, particularly their view that there was a sudden speeding-up of development between the ages of 10 and 11, and it was this which stimulated the present inquiry.

II. METHOD OF EXPERIMENT

A. Tests

The results of four tests given to the children of an urban elementary school of ages 5-13 inclusive are recorded here.¹ The purpose of the first was to trace the growth of the understanding of everyday time-words, while that of the other three was more particularly

¹ Two other tests, a 'future autobiography' and a 'making an appointment' test, were carried out, the object of the first being to elucidate further the conception of a period of years, and that of the second to discover the relative importance of time and place to the child. The results were so unsatisfactory that they are not reported.

to explore the development of the child's grasp of the universal and continuous nature of the time-scheme.

(1) *The 'Questions' test*

This was given as a group test to children of ages 8-13 inclusive, and as an individual test to children aged 5, 6 and 7 years.

1. "What is your age?" For younger children tested individually the form was "How old are you (Peter)?"

2. (a) "When you are (age + 4 years) will you be older or younger than you are now?"

(b) "Will your Mother be older, younger, or the same as she is now?"

(When testing a young child individually 4 years was added to the child's age. Thus, for a child aged 5 the question was "When you are nine . . . ?" In group testing 4 or 5 years was added to the average age of the class.)

3. (a) "Is it morning or afternoon now?"

(b) "How do you know?"

(If the test was given in the morning, the above form was used. If in the afternoon, it was altered to "Is it afternoon or morning now?" in accordance with Terman's procedure (23). This was a precaution against the tendency of some children always to choose the latter of two alternatives.)

4. "Do you come to school on Saturdays?"

5. "What day of the week is it?" (With younger children, "What is the name of to-day?" was added if necessary.)

6. "How many days are there in a week?"

7. "What are the names of the days of the week?"

8. "Does to-morrow come after yesterday or before yesterday?"

9. "What time is it now, by the clock?" (Before giving this question, the writer made certain that the clock had been removed, or was out of the sight of the subject.)

10. "What time, by the clock, is it for your Mother at home now?"

11. "What is the date of your birthday? Write down the month and the day of the month."

12. "At what time does school finish in the afternoon?" (For younger children: "What time do you get out of school in the afternoon to go home?")

13. "What time is it at (Sumner) now?" (The younger children were first asked if they had ever been to Sumner, a sea-side resort about 8 miles from the city. If they had not, the writer inquired from them a place some distance from the city where they had been, and put the question in the altered form.)

14. "What month is it?" (For the younger child, "What is the name of this month?")

15. (a) "What season is it now?"

(b) "How do you know?"

16. "Do you all know who Robin Hood was?" (If the answer was a chorus of 'No' a few explanatory remarks were made about him, such as, "He was a robber and lived in the forests in England.") Then: "He lived in the year eleven hundred and eighty-seven (1187)."

(a) "Was your Mother alive then?"

(b) "Was your Grandmother alive then?"

17. "Was Jesus alive on earth then?"

18. "How long will it be till you are a grown-up?"

19. "What time does school start in the morning?"

20. "What year is it?"

21. "How long have you been at school? Write down how long it is since you first started school."

22. "What day of the month is it? What date is it?"

23. "What is the name of the seventh month of the year?"

24. "How long is it since the Term holidays?"

25. "How long do you sleep each night?"

26. "How long will it be until the holidays begin again?" (The following explanation was always given: "Think when your *next* holidays will be. How long will it be until they begin?")

27. "What year were you born in?"

28. (a) "How long do you think it would take you to walk round this room?" (After answering, the child was asked to walk round the room.)

(b) "How long did it take you?" (With group testing, it was necessary to ask only one child to walk round the room, (b) being altered to, "How long do you think it took him?")

29. "How do we tell the time?"

30. "How long have I been in the room giving you this test?" (For individual testing, "How long have you been sitting down here talking to me?")

(2) *The 'Picture-Arrangement' test*

The particular object of this test⁽¹⁾ was to find whether young children had any conception of a long period of years. In this way it is supplementary, or introductory, to the next test. It was given individually to children aged 5, 6 and 7 years.

Five reasonably large pictures representing a baby, a boy, a youth, an adult, and an old man respectively were placed on a small table before the child. He was told "Here are some pictures. Put them in a row, first the picture of the youngest person here (point to a spot a little to the left of the child's left hand), then the picture that shows the person who is a little older, and then the next older, the next older, and the oldest at this end (pointing to different places from left to right successively)."

(3) *The 'Order-of-Dates' test*

This comprised three sub-tests.

(a) *The 'Nero-Attila-Philip' test:*

There was written on the blackboard:

Attila lived in Hungary in A.D. 438.

Philip lived in Spain in A.D. 1585.

Nero lived in Rome in A.D. 50.

This was read out, and the class instructed to "Write down the names of the people in the order in which they lived, beginning with the one who lived longest ago—furthest away back in history."

In explanation, a few further remarks were made, such as: "There are the three names, Attila, Philip and Nero, but Attila lived in Hungary in A.D. 438, Philip lived in Spain... etc." The instructions were repeated and the class told to write only the three names, one under the other, and not the whole of each sentence.

(b) *The 'John-Mary' test:*

On the blackboard was written:

John was born in 1898.

Mary was born in 1901.

Who is the older?

This was read aloud to the class, with the further instruction "I want you to write down just the one name, either John or Mary, whoever is the older."

(c) *The 'King Alfred-Joan of Arc-Captain Scott' test:*

Captain Scott was born in 1868.

King Alfred was born in 849.

Joan of Arc was born in 1412.

The instructions were similar to those in the 'Nero' test.

The writer first ascertained whether all the children knew who these persons were, Captain Scott being explained as the polar explorer who died at the South Pole.

It had been suggested to Sturt and Oakden that the results of the 'Nero' test were influenced by the factors of unfamiliarity of names and remoteness of dates. Hence they devised the 'John-Mary' test. They appear, however, to have overlooked the fact that as it includes only two names, mere guess-work would result in a 50% score and thus the value of these results is doubtful.

The 'King Alfred-Joan of Arc-Captain Scott' test was devised in order to serve more adequately the purpose of the second test and to give a more accurate check on the influence of the factors of remoteness and unfamiliarity.

The above three sections of the 'Order-of-Dates' test were presented seriatim. As a group test, it could be given satisfactorily only to children of 8 years and upwards.

(4) *The 'Temporal-Absurdities' test:*

Although this test differs from that devised by Sturt and Oakden, most of the absurdities included in their form are incorporated in it.

The first section relates to the discovery of New Zealand by the Maori explorer Kupe, about a thousand years ago. Included in it are a number of the Sturt-Oakden absurdities, some changed as to months and seasons to suit conditions in the Southern Hemisphere; in addition there are a few other absurdities. The second section is a portion of the Sturt-Oakden form.

Children of ages 8-13 were tested. Before the test was given the absurdity story from Ballard (2), p. 38) was read, each absurd point being fully explained.

Sheets on which the stories were cyclostyled (without, of course, the absurdities being marked) were then distributed, and the instructions read as follows: "Here are two stories. In them are many things which are wrong, nonsense or absurd. You are to underline all the things that you can find wrong or absurd, and to write in the margin why you think them so. Read right through the test before commencing to underline."

A One summer morning in the year A.D. 900, just as the sun was setting, the first Maoris reached
 B New Zealand. They were Kupe and his companions. They voyaged down the East Coast of the North
 C Island but made slow progress owing to the shortness of the days. When they arrived at what now is
 D Wellington Harbour one Thursday evening in August, a month after their arrival at these shores they
 E saw a tribe of strange natives having breakfast. These prepared to attack Kupe and his friends, so
 F they remained on their steamer that night and kept guard. Next morning, Saturday, Kupe landed
 G about a quarter of a mile from the enemy savages. A day's march brought these to the spot and then
 H the fight began, but Kupe and his companions won because they were armed with guns. They took
 I prisoners all the other tribe and on the 31st of September took the train to Wanganui, where they
 K settled down with some other natives just in time to help them harvest their autumn crops. Kupe's
 L grandson is still living there to-day.

In 55 B.C. Julius Caesar arrived with his troops at Dover. They had had a stormy crossing and now
 as the sun was sinking they splashed through the surf to the land. They camped that night, Wednesday,
 M the 30th February on the shore and kept guard against any surprise attack by the Britons. Next
 N morning they were ready for battle and at dawn Caesar offered a sacrifice. Taking off his tophat he stood
 O before the altar and prayed, "O, Lord Jesus, may this day, Friday, by others regarded as unlucky,
 P prove fortunate to us." The Romans were victorious owing to their greater skill in warfare, and the
 Q British chieftain was taken prisoner and shown in Caesar's triumphal procession 3 years later, 58 B.C.

B. Standard of marking

For some of the questions a closer degree of correctness was required of the older children, and for others limits were set to a correct reply. Table 1 shows the standard of marking for these questions, and for some others where the standard is not apparent.

In the 'Temporal-Absurdity' test the significant response was the reason given in the margin and not the underlining. Only if the correct reason were written was an underlined absurdity credited.

Table 1

Question	R (right)	W (wrong)	! (absurd) All absurd answers	V (vague)
1. Age		Not more than 2 years out	More than 2 years out	
6. No. of days in week		5-10 days	Below 5 or above 10	
7. Names		When one day missed		
9. Time now	Within 20 min. (graded)	Within 2 hr.	2 hr. out—a very different time	
12. Time school ends		Within 1 hr.	Over 1 hr. out	
18. Till grown up	When 18-30 years old (graded)	When 15-40 years old	E.g. 50 years, 100 years	
19. Time school begins	8.55 or 9 a.m.	8-10 incl.	Over 1 hr. out	
21. How long at school	Within $\frac{1}{2}$ year	Over $\frac{1}{2}$ year	E.g. 20 years	Started when 5; this year
24. Since holidays	Within 1 week		E.g. 6 months	
25. Sleep	8-14 hr. (graded)	14-24 hr. (graded)	Under 4 hr. and over 24 hr.	
26. Till holidays	Within 1 week			
28. (a) Walk room	Any fraction of 1 min., except 1 sec.	1 sec. or more than 1 min. Answers of '1 min.' marked 'X'	E.g. '5 min.' '1 hr.'	E.g. 15 times
(b) After doing so				
30. Time for test	Within 10 min. of correct time	Within $\frac{1}{2}$ hr.	Over $\frac{1}{2}$ hr. or e.g. '2 min.'	

III. RESULTS

1. The 'Questions' test

If we take 75% correct replies to indicate a reasonable grasp of the question asked, the order of difficulty is as shown in Table 2.

The results show that the 5-year olds have the merest rudiments of conventional time-knowledge. Those questions which the 6-year olds can answer all have a personal reference, as might also be said of the questions in which the 7-year olds are the first to be successful.

The next age-group masters the vocabulary of time-names generally, including a fair grasp of the calendar of months (but note that ability to calculate the seventh month is not general until 11 years and that consequently this knowledge is not very certainly founded) and of the organization of the week.

Nine-year olds can comprehend a long period of years. The group aged 10 are the first to understand the spatial extension of time over the city and its environs. Among those aged 11 the general even increase in correctness continues but there is no special advance. The last two groups, aged 12 and 13, answer most of the remaining questions, which mainly concern duration.

There is thus a definite order in which time-knowledge is acquired: first, that having a personal reference; secondly, that relating to the organization of the calendar of months and of the week; and thirdly, and most difficult, that more particularly involving the element of duration.

Within the second group "the tendency is for growth to occur outwards from... frequently recurring cycles to those of longer periodicity" (20, p. 51), i.e. the week, the

month and day of the month and the year. Questions 4, 7 and 5 relating to the week are higher in Table 2, then further down come questions 14 and 22, while question 27 (which demands a full understanding of the year and not simply its number as in question 20) comes lowest. It will be noticed that question 27 was not correctly answered by the required 75% even among the oldest children, who scored 53%. Higher scores were achieved by the 10-, 11-, and 12-year olds, but even these are poorer than the relative scores for questions 14 and 22.

The answers of many of the younger children to question 6 ('five') showed that they excluded Saturday and Sunday; for them the school week is *the* week; and in this they

Table 2

Age (Years)	Question	First percentage occurring above 75 %	
6	1. Age	95	Personal references
	3. (a) a.m. or p.m.	95	
	4. School on Saturday	85	
	2. (a) Age + 4 years	75	
	(b) Age of mother + 4 years	75	
7	3. (b) Reason a.m. or p.m.	75	Personal references
	15. (b) Reason for season	92	
	29. How tell time	84	
	16. (a) Robin Hood and Mother	80	
	15. (a) Season	76	
8	7. Names of days of week	95	Conventional names used in calendar and organization of the week
	5. Day of week	90	
	11. (b) Month of birthday	90	
	11. (a) Day of birthday	81	
	20. Year now	81	
	19. Time school starts	81	
	14. Name of month	76	
9	22. Day of month	76	Conventional names used in calendar and organization of the week
	6. No. of days in week	93	
	18. Till grown up	90	
	16. (b) Robin Hood and Grandmother	83	
10	8. To-morrow and yesterday	92	Long period of years
	12. Time school ends	77	
	10. Time for Mother	77	
	13. Time at Summer	77	
12	9. Time now	82	Spatial extension
	21. How long at school	82	
	23. Name of seventh month	82	
	30. Time talking	78	
13	28. (a) Walk room (before)	100	Duration
	24. Time since holidays	93	
	28. (b) Walk room (after)	86	
	25. Time asleep	67	
	17. Jesus alive	67	
	26. Time till holidays	60	
	27. Year of birth	53	

show a characteristic similar to that of those primitive peoples who omit from their calendar periods of rest such as those following the harvest.

The division of the day is more difficult than that of the year and week, as is shown by the answers to question 19 (year 8), question 12 (year 10), question 9 (year 12) and question 25 (year 13). This is possibly due to the relatively greater importance of the element of duration in the formation of the day-scheme. From the answers given to question 25 it is clear that there is some difficulty in fusing the hours of light and of darkness into 1 day. Day and night are separate conceptions, and the latter is often conceived

to be very short, e.g. an hour or less. This is understandable since the child drops off to sleep (or should!) soon after being put in bed and the next thing he is conscious of is that it is morning, and the impression is created: "A short time after I go to sleep it is morning." Among the older children there were many replies of '24 hr.' Here the day (in one sense) is known to be of 24 hr., and so also, it is believed, must be the night. Frequently the answer was '12 hr.', based on the view, presumably, that day and night (and therefore equally) together constitute 1 day: these answers have been scored separately (X) for the last three age-groups.

More than half of the 5-year olds know that a minute is a short period of duration, as can be seen by adding the right and wrong scores (including 'minute' answers marked 'X') to question 28 (a) and (b). Until 8 years there is little use of the second, which is negatively confirmed by the high 'X' scores for years 5, 6 and 7 in question 28 (b) compared with the sudden decrease in year 8.

Arithmetical ability is an important factor in the growth of time-knowledge generally, but particularly with reference to the estimation of duration and the statement of that estimate (e.g. question 28). Questions 6, 21, 23 and 27 all demand its application.

Two other questions require special notice, questions 8 and 17. 'To-morrow' and 'yesterday' (question 8) are unique in that they are the only time-words which are both relative and precise ('presently' and 'recently', 'older' and 'younger' are not precise). The form in which the question was asked, both relative terms being included, would seem to account for its location for a correct reply in the 10-year group, since other writers noticed the words to be used correctly separately at earlier ages ((7), p. 83; (18), p. 178; (19), p. 383; (21), p. 456).

Question 17 was so designed as to forestall replies based on theological dogma, but this seems to have influenced most children in view of the low score even for the 13-year olds. It is possible that many children believe Jesus to be alive on earth now; e.g. the writer was told of a small girl who asked "Does Jesus live up on the hills or down on the flat?"

2. The 'Picture-Arrangement' test

The results are set out in Table 3 which also records, for comparison, the scores of the same children for question 18.

Table 3

Age (years)	5	6	7
No. tested	18	20	19
Percentage correct	0	20	79
Question 18—percentage correct	0	20	64

Similar responses were made to each problem, from which it appears that not until 7 years does the child attain any conception of a long period of years.

3. The 'Order-of-Dates' test

The results for the three sub-tests are set out in Table 4, and show that the second and third tests were definitely easier.

Table 4

Age (years)	...	...	...	...	8	9	10	11	12	13
No. tested	...	...	...	...	21	29	27	34	47	19
Percentage correct										
Nero-Attila-Philip					19	48	56	82	85	63
John-Mary					33	59	93	85	94	89
King Alfred-Joan of Arc-Captain Scott					24	48	70	91	91	94
Average					25	53	73	86	90	82

It must not be overlooked, however, that in the 'King Alfred' test, the very fact that the names were familiar and the scores higher might mean that some of the children were able to answer correctly by thinking in terms of epochs, and not the chronological order. Consequently, the results of the 'Nero' test are probably more accurate. The drop in the 13-year olds' score in the first and second tests and generally their unexpectedly poor scoring in other tests, e.g. question 27, are due to the unrepresentative character of their group. The brighter children of this age are already at secondary school.

Fig. 1 records Sturt and Oakden's results for the 'Nero' test, the present results in the same test and in the 'King Arthur' test. Sturt and Oakden record an abrupt increase of ability between the ages of 10 and 11 while the ideal even and gradual development is

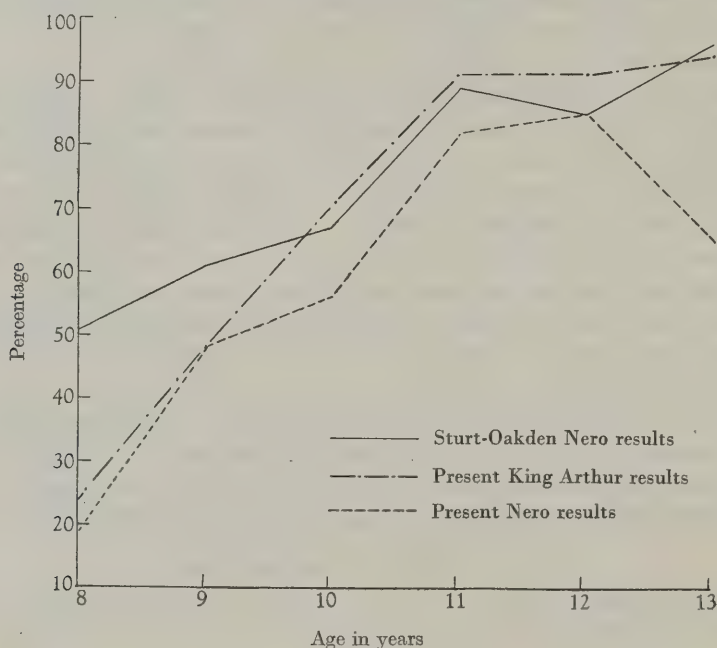


Fig. 1.

shown in the 'King Arthur' results. The present 'Nero' results also record a marked increase in the score between 10 and 11, but this increase is a continuance of the general rise between 8 and 9 and does not bear the same relation to the previous line of the graph as is shown in Sturt and Oakden's results. Further, if an ideal line were taken from the score for age 8 to that for age 11 in both cases, it would be seen that the divergence of the actual line would, in Sturt and Oakden's results, be on one side only of that ideal line, while in the present 'Nero' results it would be on both sides, indicating the expected interference of accidental factors. The present 'Nero' results are therefore shown not to contradict, and to some extent to confirm, the view based on the 'King Arthur' results that development is even and continuous. A negative confirmation of the present finding is provided by Sturt and Oakden's own results when they gave a test similar to the 'Nero' test, using the names of Plato, Burke and Swift, to the same children excluding a certain slum school. There they found that the sudden rise in score occurred between ages 9 and 10, a year earlier than in the 'Nero' test.

4. The 'Temporal-Absurdities' test

Table 5 records the results and Fig. 2 represents graphically the average score for each age-group compared with Sturt and Oakden's similar scores. Again, the present results show a continuous even development, refuting Sturt and Oakden's hypothesis of a sudden increase in ability in the eleventh year.

Table 5

Age (years)	...	8	9	10	11	12	13	
No.	...	...	21	30	28	31	49	17
								Average (%)
<i>A</i>		48	50	46	74	60	82	60
<i>B</i>		5	17	28	35	48	65	33
<i>C</i>		5	3	7	29	24	24	15.3
<i>D</i>		0	23	7	13	24	24	15.2
<i>E</i>		14	27	71	71	86	88	59.5
<i>F</i>		24	33	28	48	46	41	36.7
<i>G</i>		5	3	0	13	12	12	7.5
<i>H</i>		19	27	61	68	72	88	55.8
<i>I</i>		0	0	0	10	10	0	3.3
<i>J</i>		14	37	68	90	84	82	62.5
<i>K</i>		0	7	10	45	27	29	19.7
<i>L</i>		10	23	64	71	78	82	55
<i>M</i>		0	0	4	26	24	35	14.8
<i>N</i>		14	23	61	71	66	88	53.8
<i>O</i>		10	10	21	32	36	24	22.2
<i>P</i>		19	23	32	64	60	53	41.8
<i>Q</i>		0	0	0	13	8	0	3.5
Average (%)		11.0	18.0	30.0	45.5	45.0	48.0	
Rise (%)		—	7.0	12.0	15.5	0.5	3.0	

When the separate absurdities are arranged in order of difficulty the result is as follows:

- | | | |
|---------------------|---|---|
| (1) <i>J</i> —62.5 | (took the train) | } Distinction between
past and present |
| (2) <i>A</i> —60.0 | (sun setting in the morning) | |
| (3) <i>E</i> —59.5 | (Kupe on a steamer) | |
| (4) <i>H</i> —55.8 | (armed with guns) | |
| (5) <i>L</i> —55.0 | (Kupe's grandson still living) | |
| (6) <i>N</i> —53.8 | (Caesar's tophat) | |
| (7) <i>P</i> —41.8 | (Friday following Wednesday) | |
| (8) <i>F</i> —36.7 | (Saturday following Thursday) | |
| (9) <i>B</i> —33.0 | (short days in summer) | |
| (10) <i>O</i> —22.2 | (Christ worshipped in B.C.) | |
| (11) <i>K</i> —19.7 | (autumn crops) | |
| (12) <i>C</i> —15.3 | (August, a month after arrival in summer) | |
| (13) <i>D</i> —15.2 | (breakfast in the evening) | |
| (14) <i>M</i> —14.8 | (30 February) | |
| (15) <i>G</i> — 7.5 | (day's march to go $\frac{1}{4}$ mile) | |
| (16) <i>Q</i> — 3.5 | (58 B.C., three years after 55 B.C.) | |
| (17) <i>I</i> — 3.3 | (31 September) | |

A distinction can be made between those absurdities involving a general differentiation between past and present (which are highest on the scale), those relating to natural phenomena and personal activity (which roughly tend to precede the next group) and those concerning conventional time-names.

This order is not rigid for very many reasons, e.g. *A* is the first to occur. *P* and *F* involve the days of the week, which are known to all 8-year olds (who were the

youngest tested, and *vide* 'Questions' test results). Nevertheless, the general order of development as shown in the 'Questions' test results is confirmed.

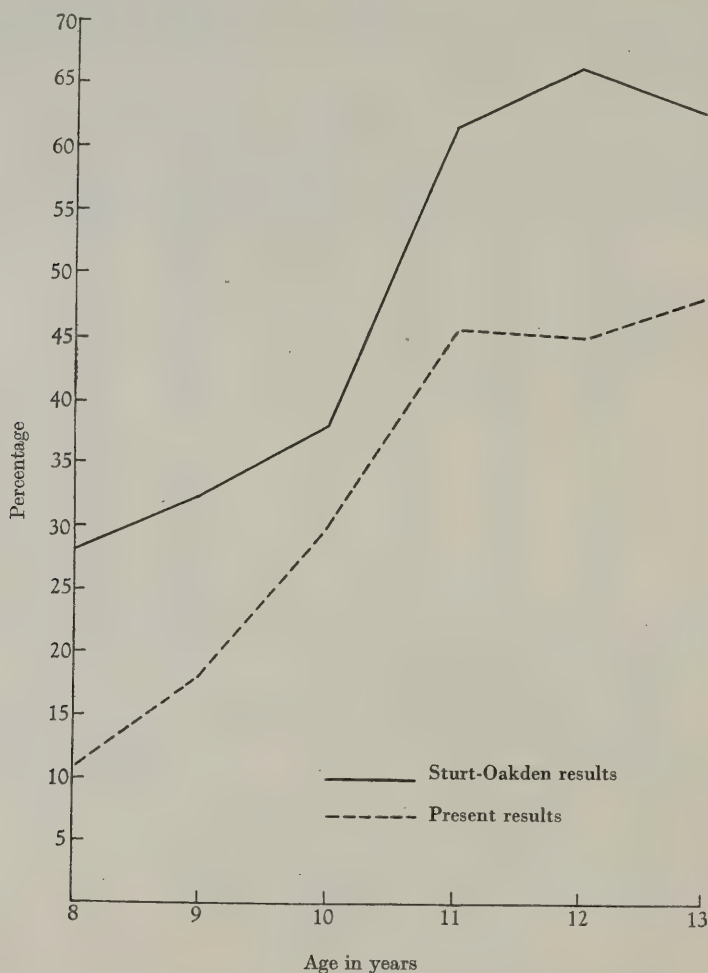


Fig. 2.

Table 6

Age (years)	...	8	9	10	11	12	13
<i>L</i>		10	23	64	71	78	82
16 (<i>b</i>)		71	83	88	83	98	93

In tabulating the scores, girls' and boys' results were separated. On the whole their scores were approximately equal, except in four absurdities where the boys' superiority was marked. These were *E*, *H*, *J* and *N* where the points involved related more to a boy's interests than to a girl's.

Comparison of the results for some of the absurdities with those for others and those for the 'Questions' test brings out further points.

Table 6 records the results for Absurdity *L* and question 16 (*b*).

Three factors seem to account for the result in this case: namely, (1) guesswork would result in a 50% score in the question, (2) the question has a personal reference, (3) the absurdity is positively suggested.

In marking question 7 of the 'Questions' test, the writer noticed that Thursday was the day most difficult to remember. Comparison of Absurdities *F* and *P* confirms that while this may be true for ages 8 and 9, apparently it is not so thereafter.

Table 7

Age (years)	...	...	8	9	10	11	12	13	Average (%)
<i>F</i> (Saturday-Thursday)			24	33	28	48	46	41	36.7
<i>P</i> (Friday-Wednesday)			19	23	32	64	60	53	41.8

The relative scores for Absurdities *I* (the most difficult) and *M* show a similar difficulty in remembering the later parts of a rote-learned table. A factor contributing to the different scores is that here February is attributed 30 days, whereas in the infant's rhyme special emphasis is made on "28, excepting Leap Year 29".

Again, *O* was very much easier to detect than *Q*, confirming one's observations among adults that it is one thing to understand the meaning of 'B.C.' and another to understand the principle on which B.C. dates are ordered; B.C. dates are, after all, comparable to mirror-writing.

IV. SUMMARY

It is clear that any inquiry utilizing quantitative methods as in the present investigation should be checked by systematic observation of individual children. Records of the progressive use and understanding of time-words by children of somewhat superior general ability have been made by several observers, but most of them stop short at the age of 5 years. The present results show that at this age the average child's understanding of the conventional time-scheme is very meagre, so far, at least, as being able to express his knowledge in answer to questions is concerned. That the children of this age knew at least vaguely the general sense of the questions asked is shown by the fact that there are very few questions in which no right or wrong answers were given. In the next 2 years progress is so rapid that every question was answered correctly by some children of the 7-year old group.

The results indicate that after the age of 5 there is a general order of development in which definite stages are successively achieved. Already the distinctions between the present, an undifferentiated past and the future have been made and thereafter the first time-words to be used have a reference to natural phenomena and personal activities or relationships. Then come the words of a more purely conventional nature and here the tendency is for the names of the more frequently recurring cycles to be understood before those of longer periodicity, with this qualification, that the day is understood first and thereafter succeeds knowledge of progressively shorter and longer periods.

In general, the capacity to understand the conventional time-scheme and to use particular time-words correctly is later in developing than is usually believed, and this is of major significance, particularly in relation to the teaching of history.

In particular Sturt and Oakden's conclusion that there is a sudden access of time-knowledge between the ages of 10 and 11 years is not confirmed, and the process is seen to be essentially gradual, even, and continuous.

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DIFFERENT TYPES OF PERCEPTUAL ABILITY¹

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I. *Introduction* (p. 79). II. *Preliminary experiments* (pp. 79–81). III. *Experiment A* (pp. 81–83). IV. *Experiment B* (pp. 83–86). V. *Discussion* (pp. 86–88). VI. *Summary* (p. 88). *Appendix* (pp. 88–89). *References* (p. 89).

I. INTRODUCTION

The investigation to be described in this paper originated in the discovery of apparent discrepancies found to occur not infrequently between sensory acuity and perceptual skill. Thus R.A.F. pilots, who were known to have good visual acuity, nevertheless showed in some cases a lack of efficiency in actual operations depending upon the visual function (1). Similarly, aptitudes tested by sensory-motor tests were found to have no relationship to the job proficiency of anti-aircraft personnel (2). It had been shown previously by Craik and the present writer (3) that 'night vision'—that is to say, the perception of objects under low degrees of illumination—was dependent only in part upon the absolute threshold for vision; and was also influenced by various factors connected with perception and identification of such sensory patterns as could be discriminated. It seemed not unlikely, therefore, that visual acuity and the differential threshold for brightness were not perhaps the only, or even the main, factors in determining perception of forms and objects in ordinary daylight vision. Some preliminary experiments carried out by Grindley appeared to show a positive relationship, but one that was not statistically significant, between the differential threshold and the tachistoscopic perception of pictures (4). But certain features of the experimental technique qualified the general validity of this result.

A further analysis of the problem suggested that 'perceptual skill' or 'perceptual ability' might not be a unitary factor. It seemed possible that the perception of certain types of material under certain conditions—for instance, the noting of fine details of shape—might be more closely related to the differential threshold, whereas the perception of more complicated material of a representational or meaningful character might be qualified only to a very slight degree by the threshold. Neither could it be assumed that individuals who excelled in the first type of task would also excel in the second, or vice versa. Again, previous training and experience might exercise a differential influence. Also the speed factor, and the amount of memorization required, might operate differently with different individuals.

II. PRELIMINARY EXPERIMENTS

It appeared to be somewhat difficult to analyse out and control separately all these different possibilities of variation. Therefore, some rather general and uncontrolled experiments were first carried out, in order to map out the field, before proceeding to any exact determination of the relationship between the different types of perceptual ability, and their dependence on the differential threshold. In these preliminary experiments, several fairly complex sets of material were used, comprising: representations of objects jumbled together; coloured pictures; geometrical patterns; more complicated curved patterns; graphs of various kinds; and diagrams of mechanical models. These were displayed on

¹ This paper is based upon one read to the Experimental Psychology Group of the British Psychological Society on 10 April 1947. The work was carried out for the Medical Research Council's Applied Psychology Unit at the Cambridge Psychological Laboratory.

large sheets of cardboard and shown for periods of time of the order of 0.5 min. to thirty-two subjects, either singly or in small groups. These subjects, W.A.A.F.'s and research workers, were also tested with Progressive Matrices and with Blackburn's Visual Perception test—a test for *k*-factor. It was found that there was no relationship between the ability to reproduce correctly these different types of material, except between the number of objects correctly identified when jumbled together and when shown in connected pictures, where, as shown in Table 1, χ^2 was just significant. Nor was there any correlation with intelligence test or *k*-test scores. It was also found that the methods used in perceiving and reproducing differed for different types of material; and also differed for different subjects with the same types of material. Sometimes two subjects might obtain almost identical scores using quite different methods, for instance, the visual imaging or the verbal description and remembering of the material.

Table 1¹

Objects correctly remembered when arranged in pictures	Objects correctly remembered when jumbled together	
	No. of subjects scoring	
	Above average	Below average
Above average	6	3
Below average	4	11
Total	10	14

$\chi^2=3.70, P=0.05$ approx.

¹ Eight of the subjects in this group were tested with material different from the others.

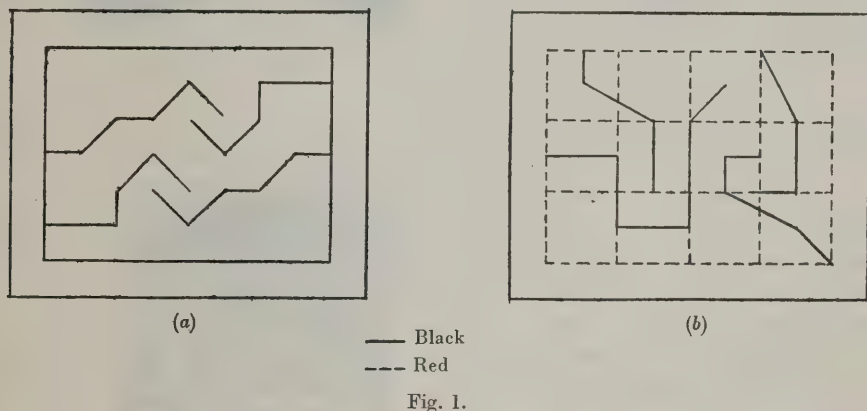
It seemed therefore that different types of perceptual process might occur which varied both according to the nature of the material, and also according to the subject's individual characteristics. The latter again seemed to vary with inherent differences of ability in visualizing and in verbal analysis and remembering. Thus those who were poor visualizers might be deficient in remembering pure shape characteristics but might compensate for this whenever they were able to analyse the material into its constituent parts and relate these to some logical verbal classification. Also particular experience and training in the use of, for instance, geometric and diagrammatic material, had considerable influence on these individual differences.

Further experiments were carried out on the remembering of diagrammatic material. Two examples of this are shown in Fig. 1. The essential feature of the first example, (*a*), was the symmetrical repetition of the patterns, and it was found that the subjects (forty-eight school-girls, aged 15–17, and twenty-four R.A.F. aircrew in training) differed considerably from one another in their ability to note and reproduce this symmetrical repetition. The essential feature of the second example, (*b*), was that the black lines were arranged upon a red 'grid'. The subjects were given a similar 'grid' upon which to record their reproductions. Again there were individual differences in the ability to perform this task. A barely significant correlation of 0.31 was obtained between the accuracy in performing these two tasks. It seems possible therefore that good 'pattern' memory is not necessarily related closely to the ability to make use of a verbally remembered 'framework' such as that given by relationship to the 'grid reference'.

Nevertheless, here again particular non-visual aptitudes, experiences and training played a part. The average scores of those girls who did well in School Certificate mathematics was significantly higher than those of girls who did less well; while the average

scores of those not considered sufficiently proficient in mathematics to take it in the School Certificate, were lowest of all.

In these experiments it was impossible to differentiate between the effects of immediate perception, and of remembering over a period of time sufficient to reproduce the material. Thus the abilities just noted may have related to methods of efficient remembering rather than to accurate immediate perception. Consequently, it was decided to use simpler material which could be reproduced quickly; and which also was probably less affected by differences in individual training and acquired skill. In order to ensure immediacy of perception, the material was exposed tachistoscopically.



III. EXPERIMENT A

For convenience in obtaining results, three different tachistoscopes were used for experiments A and B: (1) Grindley's short exposure tachistoscope which is monocular, and has an exposure period of a fraction of a second (5); (2) the Dodge type binocular tachistoscope constructed by Oldfield and Grindley (6), giving an exposure of 1-2 sec.; (3) a new tachistoscope designed by Grindley and used by him for the experiments mentioned above (4), in which a slowly opening shutter gradually lights up a figure superimposed on a mottled grey background which has a 'dazzle' effect on the outlines of the figure (for fuller description, see Appendix on p. 88). The subject has to report what the figure is at the lowest possible illumination. This tachistoscope is convenient to use in that it can also be employed for measuring the differential threshold for brightness. A white background is substituted for the mottled grey one, and for the figure, a piece of card divided longitudinally or horizontally into equal black and white parts; the subject has to report which is the lighter half as soon as he can see it. But the exposure conditions of this tachistoscope differed markedly from those given by the other tachistoscopes, and so also, probably, did the type of perceptual process in operation. This was shown by the results of experiment A, obtained from thirty R.A.F. aircrew in training.

In this experiment, the material used consisted of (a) simple meaningless outline diagrams, (b) capital letters and numbers, (c) silhouetted geometrical shapes to be differentiated from one another, (d) black and white pictures taken from the *Radio Times*. Specimens are shown in Fig. 2. Ten examples of each of these types were shown in the 'dazzle' tachistoscope, and ten in the short-exposure tachistoscope. The scores for the

former were given by the average illuminations in foot-candles at which the diagrams, etc. were first perceived. For the latter, the scores were given by the number of diagrams, etc. correctly perceived. These scores were inter-correlated, and the results are given in Table 2.



Fig. 2

Table 2¹

Material shown in dazzle tachistoscope	Differential brightness threshold	Material shown in dazzle tachistoscope				Material shown in short-exposure tachistoscope			
		(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
(a)	0.705	—	0.921	0.789	0.478	0.542	0.051	0.118	0.343
(b)	0.619	0.921	—	0.659	0.780	0.422	0.209	-0.050	0.312
(c)	0.733	0.789	0.659	—	0.690	0.053	0.073	0.220	0.379
(d)	0.542	0.478	0.780	0.690	—	0.136	0.464	0.080	0.164
Average	0.655	0.753				0.195			
Material shown in short-exposure tachistoscope									
(a)	0.501	0.542	0.422	0.053	0.136	—	0.284	0.358	0.254
(b)	0.134	0.051	0.209	0.073	0.464	0.284	—	-0.240	0.199
(c)	0.524	0.118	-0.050	0.220	0.080	0.358	-0.240	—	0.407
(d)	0.058	0.343	0.312	0.379	0.164	0.254	0.199	0.407	—
Average		0.195				0.228			

¹ Significant correlations in italics

It can be seen that correlations between results obtained for the different types of material exposed in the dazzle tachistoscope were all high, averaging 0.753; whereas those between the different types of material shown in the short-exposure tachistoscope were all low, averaging 0.228. In addition, it appeared that:

(1) The correlations between the differential brightness threshold and the four types of material shown in the dazzle tachistoscope were high, averaging 0.655.

(2) The correlations between the differential brightness threshold and the diagrams, (*a*), and silhouettes, (*c*), shown in the short-exposure tachistoscope average 0.512; whereas those between the differential brightness threshold and the letters, (*b*), and pictures, (*d*), shown in the short-exposure tachistoscope averaged 0.164.

(3) When the results for each set of material shown in the short-exposure tachistoscope were correlated with those for the *same* type of material shown in the short-exposure tachistoscope, the figures obtained were: (*a*) 0.542, (*b*) 0.209, (*c*) 0.220, (*d*) 0.164. The first alone is significant.

(4) Correlations between the different types of material shown in the short-exposure tachistoscope were almost all insignificant, averaging 0.228.

The conclusions to be drawn from these results appear to be, first, that in certain situations the perceptual performance is related fairly closely to the differential brightness threshold. Thus in the dazzle tachistoscope an important factor in determining the perception of every type of material was constituted by the ability to differentiate between areas of lighter or darker grey or black; upon this differentiation was based the discrimination of outline of shapes and pictures. Hence also the relationships originally obtained by Grindley(4). We may therefore infer that in so far as any perceptual task stresses to a high degree the discrimination of small differences of light and shade, then ability to perform that task will be to a considerable extent, and possibly to a major degree, determined by the differential brightness threshold.

Secondly, the differential brightness threshold exercised a considerable influence on the perception of the meaningless shapes—sets (*a*) and (*c*)—shown in the short-exposure tachistoscope, but not of the meaningful ones—sets (*b*) and (*d*). Thus we may suppose that the discrimination of minute detail was more important for the shapes than for the meaningful representational type of material, under the conditions in which the latter was shown in the short-exposure tachistoscope. But the importance of the differential threshold in perceiving must depend, not only on the type of material shown, but also upon the manner in which it is exposed.

Thirdly, the low correlations between the different types of material shown in the short-exposure tachistoscope showed that there was no other common factor in operation, such as speed of perception. Nevertheless, the processes of perceiving the same types of material in the two different tachistoscopes were so different from each other that the correlations were insignificant except in one case. It appears then that perceptual ability may be specific to particular modes of presentation, as well as to particular types of material.

IV. EXPERIMENT B

It is now interesting to consider what factors other than the differential brightness threshold entered into the perception of the various types of material. Some evidence on this point can be obtained from the experiments just described, by partialling out from the inter-correlations of the results obtained with the dazzle tachistoscope of the influence

of the differential brightness threshold. These results are shown in Table 3. There were fairly high partial correlations between (1) the outline diagrams and the geometrical shapes; (2) the letter and the pictures. But there were also high correlations between (3) the diagrams and letters and (4) the geometrical shapes and the pictures. Thus certain other factors entered into the determination of these results, the nature of which it was difficult to assess.

Table 3

$r_{ab}=0.870$	$r_{bc}=0.017$
$r_{ac}=0.523$	$r_{bd}=0.673$
$r_{ad}=0.178$	$r_{cd}=0.513$

Clearer indications were however given by a series of results obtained in experiment B with the binocular tachistoscope. This instrument allowed not only binocular vision, but also a longer exposure and a larger visual field. Thus perception approached more nearly to normal conditions of vision. The material shown consisted of:

- Pairs of outline geometrical shapes to be differentiated from each other.
- Pairs of more complicated curved shapes, also to be differentiated.
- Capital letters and numbers indicated by dots.
- Scales and dials each with a pointer the position of which was to be read.
- Blurred silhouette pictures, seen through a ground glass screen, of men, animals, etc.
- Black and white pictures taken from the *Radio Times*.

Examples of these are shown in Fig. 3. In general, the score was the number seen correctly; but other scores, to be described below on p. 86, were used for (f). The subjects, thirty R.A.F. aircrew in training, also were given A.H.4 and the N.I.I.P. Form Relations test.

Table 4¹

Material	Material						A.H.4		Form relations	
	(a)	(b)	(c)	(d)	(e)	(f)1	(f)2	Total		Part II
(a)	—	0.614	0.162	0.170	0.572	0.212	0.306	0.219	0.242	-0.054
(b)	0.614	—	-0.008	0.182	0.549	0.215	0.528	-0.056	0.060	0.120
(c)	0.162	-0.008	—	0.459	-0.017	0.112	0.197	0.309	0.474	0.572
(d)	0.170	0.182	0.459	—	-0.179	0.443	0.353	0.426	0.454	0.360
(e)	0.572	0.549	-0.017	-0.179	—	0.172	0.311	-0.206	-0.144	0.142
(f)1	0.212	0.215	0.112	0.443	0.173	—	0.461	0.209	0.262	0.057
(f)2	0.306	0.528	0.197	0.353	0.311	0.461	—	0.465	0.392	0.341

¹ Significant correlations in italics

When the results of this experiment were inter-correlated, the figures given in Table 4 were obtained. They fell roughly into two groups:

Group 1 contained the two sets of shapes to be differentiated from each other, and, fairly closely related, the blurred silhouettes; the average correlation was 0.585. These showed no relationship to intelligence or *k*-test scores.

Group 2 contained the letters and numbers, the scales and dials, and, correlated with the latter but not significantly with the former, the pictures from the *Radio Times*. The first two of these correlated with the non-verbal intelligence test scores (A.H.4, Part II), but not with the verbal, nor in general with the *k*-test scores. The pictures correlated significantly with the total scores on A.H.4. The average of the significant correlations was 0.409.

Thus it could be concluded that the perception of the shapes and silhouettes depended on some common ability unrelated to intelligence or *k*-factor. Unfortunately it was not

possible at this time to measure the differential brightness threshold, which may have been the common factor. Or the latter may have been some perceptual ability to discriminate accurately the details of form and shape. The perception of the blurred silhouettes,



Fig. 3.

as well of the meaningless shapes, must have depended to a considerable extent upon the discrimination of fine details of shape; but the latter factor was not important in reading the scales and dials, or the letters and numbers.

In the perception of the material in the second group, which was more or less completely unrelated to that of the first, it appears that some process of thinking or reasoning out

what the shapes indicate was essential. This was more closely related to non-verbal than to verbal intelligence test performance, presumably because little if any of the type of reasoning used in verbal intelligence performance occurred, except perhaps with the pictorial material. The perceptual and thought processes were not quite the same with all these types of material, however. Reading the shapes and dials correlated significantly, by about 0.45, with the perception of the letters and numbers, and pictures, but the inter-correlation between the latter was not significant. Thus the perceptual and thought processes necessary to the interpretation of the pictures, although they required some intellectual ability, differed radically from those needed for making out the letters and numbers from the isolated dots. The latter probably also required some ability to manipulate shapes imaginably, since this test alone of all others correlated significantly with the *k*-test.

The performance with the pictorial material was scored in two ways giving somewhat different results, and indicating two types of cognitive process in operation. In the first way of scoring ((*f*)1 in Table 4), the number of objects, etc. mentioned in the subject's report was counted; in the second way, ((*f*)2 in Table 4), the number of pictures was ascertained the meaning of which was entirely misunderstood. The pictures were, some of them, rather bizarre in nature, and hence required some degree of imagination to interpret. These two scores correlated by 0.461 only, showing that there must have been differences in the ability to notice separate objects, and to interpret meaning. The first score showed a greater correlation than the second with the score on reading scales and dials; but the second score showed a greater correlation than the first with intelligence, and also with the perception of shape differences. It may be therefore that interpretation of the meaning of pictures required an accurate perception of the salient detail which gives a clue to the meaning of the picture, as well as requiring more intelligence than does the mere naming of objects. For instance, the picture shown in Fig. 3 (*f*), which is not very clear in outline, was interpreted variously as: "A policeman, a little party somewhere in a room"; "A fat policeman arguing with a number of gentlemen in top hats, and another man sitting at a table in the centre"; "A few men pleading with a policeman for some one of their comrades who has been hit over the head by a bottle"; "A policeman with two men sitting on the right, and a woman supporting a child in her arms." In another picture, the head and arms shown in the foreground were frequently missed, or interpreted as a spear, the trunk of a tree, etc. The influence of the rather bizarre atmosphere created by the pictures and the imaginative effort called forth to interpret them correctly was shown in the case of the final picture. Although it showed quite an ordinary scene of a pilot in the cockpit of an aircraft, yet nine out of the thirty aircrew completely misunderstood it and called it "a man climbing up something", etc. Thus the ability for imaginative interpretation of pictures is not determined simply by the ability to name the objects in them.

V. DISCUSSION

It is interesting to compare these results with those obtained by Thurstone in a recent factorial analysis of perception⁽⁷⁾ in a large range of tests and with a large range of test material. Rather unfortunately these consisted mainly of a conglomerate assortment of the usual laboratory material of visual illusions, Gottschaldt figures, constancy effects, and so on, and contained few perceptual tasks more commonly encountered in ordinary life, such as pattern discrimination, scale reading and the interpretation of ordinary pictures. One

of Thurstone's tests, however, was utilized in experiment B, which has just been described—namely, the reading of letters and numbers indicated by dots. Thurstone extracted, among others, two main factors which he thought to be of particular importance in perception: (1) strength and speed of 'closure' and the ability to hold it against distracting influences, as shown in the Gottschaldt figures, the dotted letters and numbers, Koh's Blocks, and numbers hidden by dots; this also correlated with *k*-test scores; (2) flexibility in the perception and manipulation of competing configurations, as shown by the Gottschaldt figures, Koh's Blocks, and hidden pictures; this correlated with intelligence test scores. Now it should first be noted that the dotted letters test in Thurstone's experiments showed negligible correlations with most of his intelligence test scores (-0.210 with reasoning); in experiment B, it correlated 0.474 with A.H.4, Part II. But in experiment B, the time exposure was limited to 1.5 sec., and the subjects probably had to effect the 'closure' of imaginally held percepts (hence also the significant correlations with the *k*-test scores). In Thurstone's experiments, they named the letter into a voice key as soon as it was perceived; 'closure' thus took place during perception. Thus the procedure in experiment B may have necessitated a greater use of intelligence than did Thurstone's, whereas the sensory conditions were probably more favourable in the former than in the latter, and thus the actual perception rather easier. This difference then gives a further illustration, comparable to that instanced on p. 83, of the specificity of perceptual abilities and their dependence on the actual conditions under which perception takes place.

Apart from this, it seems possible that Thurstone's factor (1) is analogous to the perception of pure shape and discrimination of shape detail described above. Unfortunately he did not test visual acuity or the differential brightness threshold, so it is impossible to say how far factor (1) was related to these. His factor (2) may then be analogous to the ability to manipulate perceptual material, correlated with intelligence, which is necessary to extract the meaningful details and characteristics in the interpretation of pictures and the reading of scales and dials. In Thurstone's experiments, the competing configurations were meaningless except in the case of the hidden pictures. Thus it is possible that the essential feature of the second factor is constituted by the ability to pick out, from a mass of confused and competing sensory data, some pattern or *configuration determined by previous set*, instructions, etc. This set may be, as shown above, towards scale reading, or towards imaginative interpretation; or, as in Thurstone's experiments, to perceive configurations such as the simple Gottschaldt figures obscured by the more complex ones, or the hidden pictures. Or it may perhaps be constituted by an actual visual framework, for instance, a 'grid reference' such as was used in the preliminary experiments described on p. 80.

This leads to the further possibility that two of the main determinants of individual differences in perceptual ability are: (1) differences in ability to perceive simple shape as such—that is, as constituted by the more or less clearly discriminated sensory pattern; these are probably related to differences in the differential brightness threshold; (2) differences in ability to manipulate that sensory pattern intelligently in accordance with some previously established set, which determines what is perceived to a greater extent than does the simple sensory pattern as such. Now it is probable that much, if not almost all, of the perception of ordinary everyday life is affected by long-term sets or schemata. Thus differences in (2) may depend on (a) the differences in the potency, relevance and familiarity of the schemata in operation—as indicated in the first experiment described in this paper; and

(b) differing degrees of the intelligent, rather than the merely habitual, methods of utilizing these schemata. The possible variations of the types of schemata in operation, as determined by both innate and acquired factors, and of the skill with which they are utilized, are very great in number. Hence it does not seem remarkable that there should be very many possibilities of individual variation in efficiency of perception of different types of material shown under different exposure conditions.

VI. SUMMARY

1. A number of different kinds of perceptual material were shown in three different types of tachistoscope, and also for longer periods of time non-tachistoscopically. The material comprised: pictures (coloured, black and white, silhouettes), jumbled objects, letters and numbers, shapes, patterns, scales, diagrams. In some cases, the differential brightness threshold was also determined, and verbal and non-verbal intelligence and *k*-tests carried out.

2. At first sight, the impression was gained that perceptual efficiency showed a high specificity. Thus those who showed good perceptual ability with material exposed under a particular set of conditions, might show little ability with different material, or even with the same material exposed under different conditions.

3. There were some indications, however, of two general factors influencing perception, analogous to those obtained in a factorial analysis by Thurstone: (1) Ability to perceive and discriminate pure shape and pattern characteristics. This appeared to be related to the differential brightness threshold, but not to intelligence or to *k*-factor. The latter probably operates only when imaginal manipulation of the percept is required. (2) Ability to make a further interpretation of the sensory and perceptual data in accordance with some predetermined set or schema. This ability was related to intelligence, but varied according to the type of set in operation. In these experiments, three types of set could be distinguished, towards the perception of (a) representations of real objects, (b) imaginatively interpreted pictures, (c) symbolic material such as letters, numbers, scales, etc.

4. It seems doubtful, however, if it is at the moment possible to isolate unitary abilities in perceiving material of any of these types, since they are so largely affected by factors, as yet unknown, connected with the exposure conditions.

APPENDIX

The following description of the tachistoscope, shown in Fig. 4, is taken from the original description by Grindley (4):

The apparatus consisted of a rectangular box measuring 18 in. by 12 in. and 9 in. high, which is shown in plan in Fig. 4. The subject looked into this through a brass tube *T* ($3\frac{1}{2}$ in. long and $\frac{3}{4}$ in. internal diameter). A half-silvered mirror *M* mounted obliquely in front of the tube caused the subject to see both a background screen *B*, and, superimposed on this, any test figure placed on another screen *F*. In both cases the optical path from the subject's eye to the screen was 14 in., and the diameter of the field of view just over 3 in.

The illumination of both screens came from a 60 watt lamp *L* and was made sensibly uniform across the screens by the use of ground-glass windows *W*₁ and *W*₂, and white reflecting surfaces inside the two compartments. The illumination of the screen *B* could be set at any desired constant value by the use of a suitable aperture in front of the window *W*₁, while the illumination of the screen *F* could be continuously

varied by gradually opening a sliding shutter S in front of the window W_2 .¹ It was thus possible while using a plain or mottled background at B with any illumination, to cause any type of test figure F to appear superimposed on B with a gradually increasing illumination. The apparatus was calibrated for intensity of illumination (as this would appear to the subject's eye) by placing a lumeter in the position of the tube T and having one screen in complete darkness.

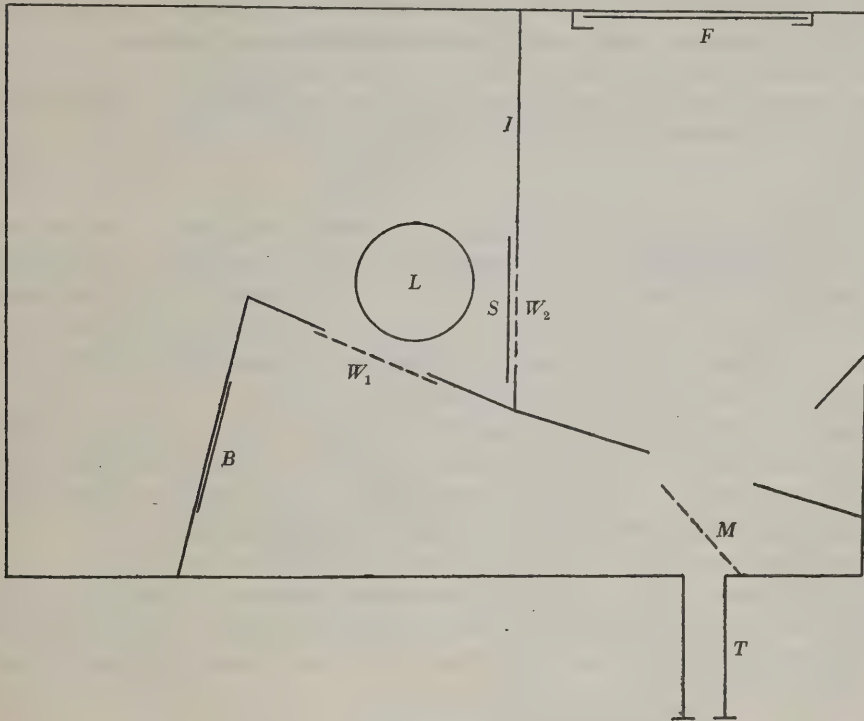


Fig. 4.

In conclusion, I should like to make my acknowledgements to the Medical Research Council, for whose Applied Psychology Group I carried out this work; and to express my gratitude to Prof. Bartlett for his assistance and advice, and for his permission to work in the Cambridge Psychological Laboratory.

¹ In the form used in experiment A, the gradual exposure was produced mechanically, S rising vertically, and therefore took place at a constant steady rate.

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HIGH INCENTIVES VERSUS HOT AND HUMID ATMOSPHERES IN A PHYSICAL EFFORT TASK

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I. *Aim of research* (p. 90). II. *Procedure with the pull test* (pp. 90-93). III. *Results of the pull test* (pp. 93-101). IV. *Summary and conclusions* (pp. 101-102). *References* (p. 102).

I. AIM OF RESEARCH

It is often stated or implied that difficult atmospheric conditions do not matter—that they do not greatly affect human performance, either (1) because the abnormal physical environment will itself act as a spur to activity or (2) because a high incentive situation (such as would arise in a battle emergency with sound leadership) will enable the men to rise above the difficulties of working in hot and humid atmospheres and so allow them satisfactorily to complete their allotted tasks.

Not many real authorities would press the first point to the extent of saying that the influence of this factor is of great consequence, but it does seem most important to examine the second assumption very carefully, to see whether in fact high incentives always win their struggle against the effect of hot and humid atmospheres.

This problem is also particularly interesting from the more general point of view of laboratory technique, since it is clearly essential to determine the extent to which the harassing effect of a given situation can be modified by altering the incentive, if the latter is to be varied over a wide range, as it usually is in many forms of human behaviour.

II. PROCEDURE WITH THE PULL TEST

The accompanying illustration (Fig. 1) shows that the basic task chosen for study was one of the simplest possible forms of work. The subject had to raise and lower a 15 lb. weight by bending and straightening his arm in time to a metronome. He was asked to continue to do this until he was completely exhausted and could not lift the weight even a fraction of an inch off the floor. Every second, the weight had to be changed from position *A* to position *B*, or from position *B* to *A*. These instructions meant that about 30 ft.-lb. of energy were being expended every 2 sec. in raising the weight, since this 15 lb. load was being raised each time to a height of 2 ft. off the floor (except when the men were rapidly tiring during the last few minutes at the very end of the test). Direct measurement of the respiratory exchange by the bag method confirmed that the task as a whole entailed heavy work, since muscular energy was being used at the rate of about 310 K.cal./hr.

As a preliminary, the subjects were given a daily practice spell on the apparatus, and during this first fortnight of the work all the men greatly improved, a ten-fold increase in their daily average being not at all uncommon. Special methods were used to obtain this sharp rise in performance, since it was convenient to remove the bulk of this practice effect from the main experiment. (a) The total amount of work done by each man for the day was marked on a chart so that each subject could compare his current score not only with his own previous attempts, but also with the scores made by other men in the group. In addition, (b) each man was set an output target figure which was based on his

previous best score. Finally, (c) on each practice day a reward of five cigarettes was given to the subject in each group who added the greatest amount of work to his previous best score.

The test procedure itself was based on some studies by Crawley (1926) who elegantly demonstrated that the amount of work done under ordinary laboratory conditions could be greatly increased by giving the subject adequate knowledge of results, i.e. by allowing him to see an ergographic record traced out on paper while the work was being done. Crawley also set a target figure for each run, this being adjusted according to the length of time the subject had continued working to make his previous best score. The target figure was indicated by a mark on the kymograph drum on which the graphic record was made. The subject was told to continue to work until he had passed this mark.

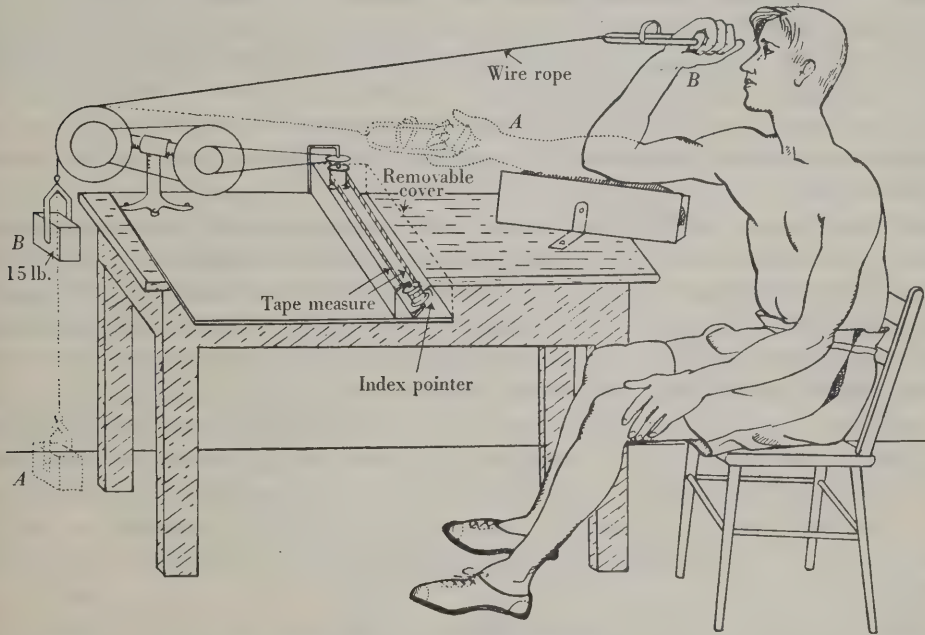


Fig. 1. Pull test apparatus.

Some modifications were made on Crawley's technique, and these are mostly shown in Fig. 1. The graphic record had certain disadvantages, not only because such records were inevitably slow to analyse, but also because the subject usually had some difficulty in determining the amount of work he had still to do to achieve the target figure. For these two reasons, the graphic record was replaced by a display which accumulated the amount of work done and so provided a continual progress record. Fig. 1 demonstrates that this was simply a tape measure 60 in. long, sewn into a continuous band and passing round two pulleys. Every time the subject raised the weight the full distance from A to B, 2 in. were added to the reading on the tape. A ratchet system arranged that lowering the weight did not alter the reading. Once round the tape therefore represented thirty complete pulls—about 1 min.'s work. The number of inches through which the tape had travelled gave an accurate measure of the work output. The target figure was more conveniently expressed as the number of times the tape had to go round plus a given number of inches, because with this type of target figure (e.g. "17 times round plus 40 in.") the subject had no difficulty in understanding the amount of work still needed to reach the target figure.

In a test run given under *high incentive conditions*, the subject was provided with a target figure which was 25% more than his previous highest score. He was asked to work all out to beat this figure and to continue working till exhausted. He could see the tape measure moving as he worked at the task, and in addition the experimenter maintained a continuous running commentary on the progress made. These comments were expressed in terms of the number of times that the tape had gone round, e.g. "1½ times round, making for 2"... "2 coming up, 2 on the way"... "2 now, making for 2½." Constant encouragement was added to this information, e.g. "Yes, that was good. You're doing well. Now make for 16." Similarly, towards the end of the run the experimenter supplied facts about the scores made by other men in the group. "You're now passing Foster"... "Once more round and you will have beaten Watts", and also added comments on the distance to the target. "Just twice more round to reach your target figure." Praise was given even more frequently as the subject neared the exhaustion point. "That's very good; you're nearly there; only once more round now." Finally, if the man beat his target figure, he was asked to see how much greater he could make his score, and, as before, the running commentary on progress was continued until he had entirely stopped working.

In a test run given under *ordinary incentive conditions*, no target figure was given, and a screen was erected to conceal the tape measure from the subject, so that he had no knowledge of results (unless he cared to count the number of pulls made, which, as far as one could tell without direct questioning, the subjects never did). The experimenter made no comments on the amount of work done, either during or after the test, and the subject worked until he felt that he could not continue.

Though a curved and padded upper arm support was provided, a good deal of latitude was allowed the subject as to the exact posture or arm movement adopted (except that the left hand had always to be kept on the left thigh as shown in the illustration). Some absorbent cloth was wrapped round the handle of the apparatus so that the presence of sweat on the subject's hand did not lead to any difficulties in gripping the handle.

Before the main experiments began, all thirty subjects had daily acclimatization spells lasting about 3 hr. each for a period of a fortnight, the room-temperatures then usually being either 100/90° or 105/95° F. on the dry bulb/wet bulb scale. The subjects were tested in six different groups of five men each. On different days each group was given one of the two levels of incentive at one of the six different room-temperatures, so that each man made altogether twelve experimental runs on twelve successive days, these being either in the morning or afternoon. On successive pairs of test days the group had one of the six room-temperatures—one day with the ordinary, the other with the high incentive conditions. For half the men the ordinary incentive day always preceded the high incentive day; for the other men these conditions were reversed. This meant that any difference between the readings for normal and high incentive conditions could not have been due to practice effects. Similarly, the six different room-temperatures were given in a different order to each of the six experimental groups, so that any difference in the readings at the various room-temperatures cannot have arisen from learning effects since the readings were taken at corresponding stages in the learning curve.

The six different room-temperatures used in the experiment were readings of 65/60°, 75/65°, 85/75°, 90/80°, 95/85° and 100/90° F. on the dry bulb/wet bulb scales. Since the

air velocity was fixed at 100 ft./min., these represented effective temperatures (E.T.) of 61, 69, 79, 83, 87.5 and 92° F. (Bedford, 1946).

The group of five men entered the room which was providing one of these six atmospheric conditions, and they remained there for 1 hr. talking, reading or sleeping before any test scores were taken. Then the pull test began and each man was tested in turn behind a screen. During the actual test the men were dressed only in shorts except at the effective temperature of 61° F. when they wore naval trousers and jerseys. In the waiting period they usually wore the jerseys and trousers at the two lowest room-temperatures; this was left to the subjects themselves to decide. The men in a group were always tested in the same order; the first two waited till the third man had finished, then all three subjects left the hot room together. In the same way, the fourth man waited till the fifth had completed his test, this being done to avoid the idea that as soon as the test was finished the subject was free to dash outside the hot room.

III. RESULTS OF THE PULL TEST

The main findings are contained in Fig. 2. When multiplied by a factor of 10, the ordinate scale in this diagram gives a direct reading of the average number of inches covered by the measuring tape, i.e. the average number of feet through which the 15 lb. weight was raised by the thirty subjects at the given room-temperature and under the stated incentive conditions. This provided a clear indication of the average amount of work achieved at the various experimental conditions before exhaustion occurred. With normal room-temperatures and a high incentive, most men could usually keep working for about 20 min., but individual differences were large and the total working time ranged between about 10 and 40 min.

The effect of room-temperature on the average amount of work done at each of the six atmospheric conditions is given in Table 1:

Table 1. *Average amount of work in arbitrary absolute units*

Room-temperature, dry bulb/wet bulb						
	65/60° F. (61° E.T.)	75/65° F. (69° E.T.)	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)	95/85° F. (87.5° E.T.)	100/90° F. (92° E.T.)
Deterioration from 61° E.T. score	82.3 units	79.9 units	73.9 units	62.5 units	56.2 units	51.4 units
		- 2.4	- 8.4	- 19.8	- 26.1	- 30.9

(1) *Incentive effects*

Fig. 2 suggests, however, that it is better to consider the data in two halves—the findings from the men when they were supplied *with knowledge of results*, and those from the same men working *without knowledge of results*.

Table 2. *Average amount of work in arbitrary, absolute units*

Room-temperature, dry bulb/wet bulb						
	65/60° F. (61° E.T.)	75/65° F. (69° E.T.)	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)	95/85° F. (87.5° E.T.)	100/90° F. (92° E.T.)
<i>With knowledge of results</i>	101.7 (K_1)	99.5 (K_2)	90.0 (K_3)	74.8 (K_4)	67.4 (K_5)	65.0 (K_6)
<i>Without knowledge of results</i>	63.0 (NK_1)	60.4 (NK_2)	57.9 (NK_3)	50.3 (NK_4)	45.0 (NK_5)	37.9 (NK_6)
Differences between <i>with</i> and <i>without</i> scores	38.7	39.1	32.1	24.5	22.4	27.1
<i>t</i> values for the differences between means	$t=4.93$	$t=3.86$	$t=3.87$	$t=2.30$	$t=1.47$ (not sig.)	$t=1.94$ (not sig.)

Table 2 confirms that there was usually a very marked difference between the average amount done by the same men depending on whether they were supplied with knowledge of results or not. The average output with knowledge of results was 83.1 units and without knowledge of results it was 52.4 units, i.e. the incentive gave a 58% increase in the amount of work achieved, so that there was no doubt that a really powerful

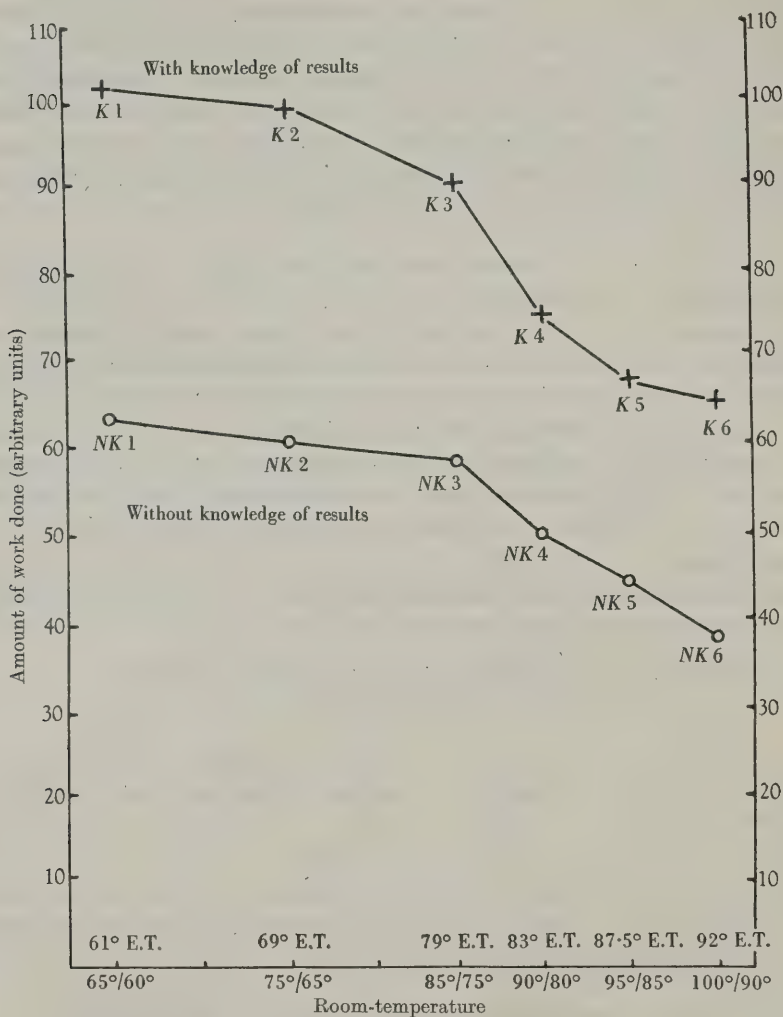


Fig. 2. High atmospheric temperatures reduce the advantages of strong incentives in physical work.

motivating factor was being introduced when knowledge of results was supplied. A further illustration of this point was the fact that when the men were receiving knowledge of results, they worked just as hard in the highest atmospheric temperature, as they did under normal atmospheric conditions when they were without knowledge of results. Lastly on 87% (156/180) of the occasions, knowledge of results gave a higher score than no knowledge of results.

(2) *Incentives and the critical level*

From this evidence it was clear that knowledge of results had a very strong influence on performance. But it was interesting to see in Fig. 2 that this powerful incentive entirely failed to prevent deterioration from occurring in the output curve at raised room-temperatures. It might have been thought that even when fairly high atmospheric temperatures were being experienced (although the low incentive curve in Fig. 2 might show deterioration) the high incentive curve would remain at or near the level of performance achieved in normal room-temperatures.

The extra incentive might alternatively have been expected to raise the critical level on the room-temperature scale at which performance began to deteriorate, given that the higher incentive could counteract rather more rigorous atmospheric conditions than the lower incentive without entirely preventing a decline.

A statistical study of the trends of the two curves in Fig. 2 showed that, on the contrary, *the critical levels on the room-temperature scale at which performance declined were exactly the same for high incentive and for low incentive conditions.*

In the output scores under the *high incentive*, the t values obtained were as shown in Table 3 for the differences between the means.

		Room-temperature			
		65/60° F. (61° E.T.)	75/65° F. (69° E.T.)	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)
High incentive	K_1	$t=0.3$ K_2			
	K_1	$t=1.2$ K_3			
	K_1	$t=3.2$ (significant) K_4			
Low incentive	NK_1	$t=0.7$ NK_2			
	NK_1	$t=1.6$ NK_3			
	NK_1	$t=3.0$ (significant) NK_4			

Since $t=2.04$ was needed for a reliable difference, this showed that at the high incentive conditions the first room-temperature at which output significantly began to deteriorate (compared with output at the normal atmospheric temperature of 65/60° F.) was 90/80° F. on the dry bulb/wet bulb scale, i.e. an effective temperature of 83° F.

Similarly for the output results obtained with *low incentive conditions* the t values were as shown in Table 3.

As before, $t=2.04$ or over indicated a reliable difference between the average scores. Therefore the first temperature at which output began significantly to deteriorate (compared with performance at the normal atmospheric temperature of 65/60° F.) was again 90/80° F. on the dry bulb/wet bulb scale, i.e. an effective temperature of 83° F.—the same as that for the high incentive conditions.

In view of the findings with other test procedures (Mackworth, 1946*b*), in which 85/75° F. was the lowest room-temperature considered, and therefore taken as the 'normal' room-temperature, it was interesting to make similar comparisons using as the optimum output the work performance at 85/75° F., instead of the score at 65/60° F.

Table 4

Room-temperature			
	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)	95/85° F. (87·5° E.T.)
With knowledge of results	K_3	$t=1\cdot4$	K_4
	K_3	$t=3\cdot7$ (significant)	K_5
Without knowledge of results	NK_3	$t=2\cdot021$	NK_4
	NK_3	$t=3\cdot3$ (significant)	NK_5

In Table 4 the critical level is placed at 87·5° E.T. when knowledge of results was supplied. Since $t=2.042$ was required for significance at the 1 in 20 level of probability, the 90/80° F. reading was very nearly, but not quite, significant and the critical level was therefore again at 87·5° E.T. without knowledge of results. In other words, the use of the performance score at 85/75° F. instead of that at 65/60° F. as a base line made only a slight difference to the critical level which was raised from 90/80° F. (83° E.T.) to 95/85° F. (87·5° E.T.). Once again the position of the critical point was not affected by a marked increase in the strength of the incentive.

(3) Incentive effects and atmospheric temperature

A glance at Fig. 2 and at Table 2 indicates that the advantage of the incentive as measured by the differences between K_1 and NK_1 , K_2 and NK_2 , K_3 and NK_3 , K_4 and NK_4 , K_5 and NK_5 , and K_6 and NK_6 became progressively smaller when this comparison between 'with' and 'without' scores was made at the higher room-temperatures. Since each point on Fig. 2 was based on readings from thirty subjects, the t value for each of these six comparisons had to be 2.04 or greater for a statistically significant difference. It will be seen in the table that the t value fell with rising room-temperature until at the two highest room-temperatures it was no longer statistically reliable; it was only 1.47 and 1.94 for the scores at the effective temperatures of 87·5° and 92° F.

This reduction in the advantages to be gained from strong incentives when the difficulty of the physical working conditions was raised was also demonstrated by grouping the data. *With* knowledge of results the average output at the three 'low' effective temperatures of 61°, 69° and 79° F. was 97.1 units; *without* knowledge of results at the same room-temperatures it was only 60.4 units. Raising the incentive had therefore led to an improvement of 36.7 units under these physical conditions. At the three 'high' room-temperatures of 83°, 87·5° and 92° F. the average output was 69.1 units *with* knowledge of results and 44.4 units *without* knowledge of results, i.e., when the physical conditions were more difficult, a smaller improvement of 24.7 units was found. The ninety readings for the increase at the low room-temperatures were compared with the ninety readings for the increase at the high room-temperatures. This proved that the reduction from 36.7 to 24.7 units in the size of the difference between the scores obtained with and without knowledge of results was statistically significant because a t value of 2.20 was obtained and a reading of 1.99 or greater meant that the odds were more than 19 to 1 against this being merely a chance effect.

It was noted in Fig. 2 that the output curves for the two different levels of incentive were approaching each other at the upper end of the temperature scale, because in each case the extent of the deterioration with higher room-temperatures seemed to be directly proportional to the output at the normal atmospheric temperature of 65/60° F.

The evidence that this was so has been shown in Table 5 and in Fig. 3. Output at the normal atmospheric temperature of 65/60° F. for the high incentive conditions was taken as 100. Similarly output at the same room-temperature for the low incentive situation was also taken as 100. In each case the points on the curve have been expressed as proportions of the output at 65/60° F.

Table 5. *Relative amount of work done at different room-temperatures*

	Room-temperature					
	65/60° F. (61° E.T.)	75/65° F. (69° E.T.)	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)	95/85° F. (87·5° E.T.)	100/90° F. (92° E.T.)
High incentive conditions	100	98	88	73	66	63
Low incentive conditions	100	96	92	80	71	60
Differences between high and low %	0	+2	-4	-7	-5	+3

It is clear that the similarity between the two curves was then very marked, as there were only slight differences between the trends, due probably to random variations in the scores arising from chance factors in the experiment.

Since the output at the higher temperatures was a fixed proportion of that at the normal temperature, the deterioration in absolute units was greater when, as in the *high* incentive conditions, the output at the normal room-temperature was larger. Therefore instead of the slope of the decline in output with rising room-temperature being more gradual with the high incentive, it was in fact steeper—as for example can be seen in Fig. 2. (The reason for this becomes clear if one considers that when the room-temperature is raised from normal conditions to 100/90° F., with either level of incentive the men are going to lose roughly one-third of their 65/60° F. output. With the *high* incentive this means a loss of one-third of 102 units, i.e. 34 units in all; with the *low* incentive it entails a loss of one-third of 63 units, i.e. a smaller loss of only 21 units.)

The hypothesis about the advantages of high incentive conditions was undoubtedly correct in that the difference between the average scores obtained at high and low room-temperatures was thereby increased. It may therefore seem strange that the critical level remained unaltered and the sensitivity of the test to high room-temperatures was not improved. The difficulty was that what one gained on the means, one lost on the scatters, because the disadvantages of the increased standard deviations of the readings found with the high incentive conditions balanced the advantages of the larger differences between the average scores when the statistical significance of the differences between the means was calculated.

(4) *Incentive effects and level of ability*

Previous investigations (Mackworth, 1946*a*) have shown that a factor in determining the extent of the performance decline resulting from high atmospheric temperatures was the general level of ability at the test under normal working conditions. It therefore

seemed necessary to divide these pull test results into two categories—those obtained from ‘good subjects’ and those from ‘average subjects’. The method used was to rank the men in order of ability on the test as shown by their scores at the normal room-temperatures of 61° and 69° F. on the effective temperature scale. The best fifteen men on this basis were regarded as ‘good subjects’ and the other fifteen men were classified as

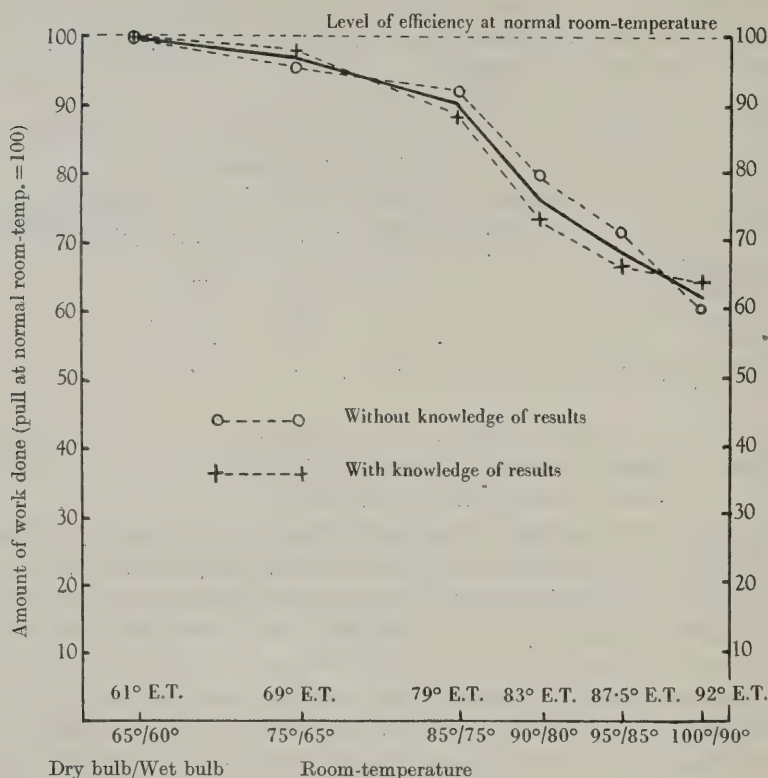


Fig. 3. The same *proportional* deterioration for high as for low incentive conditions when atmospheric temperature is raised.

‘average subjects’. (These terms were used because a few other subjects had to be rejected as unsuitable on the basis of their very low scores during the first fortnight of preliminary training before the main tests began.)

The findings have been summarized in Fig. 4 and in Table 6.

Table 6. *Average amount of work in arbitrary absolute units*

Level of ability	Level of incentive	Room-temperature					
		65/60° F.	75/65° F.	85/75° F.	90/80° F.	95/85° F.	100/90° F.
Good subjects	High	142.9	146.7	129.9	112.0	101.2	84.3
	Low	89.7	88.4	90.6	79.4	67.8	53.5
	Difference	53.2	58.3	39.3	32.6	33.4	30.8
Average subjects	High	60.5	52.3	50.2	37.7	33.6	45.8
	Low	36.4	32.4	25.9	21.2	22.2	22.4
	Difference	24.1	19.9	24.3	16.5	11.4	23.4

Taking an average over all the room-temperatures, it was found that, whereas the *good subjects* increased 41 units (from 78 units with the low incentives to 119 units with the high), the *average subjects* increased only 20 units for the same rise in incentives (from 27 to 47 units). It will be seen that the extra incentives gave twice the output increase in the good subjects compared with that found in the average men. This was also found to be true when the comparison was made separately for the incentive increase

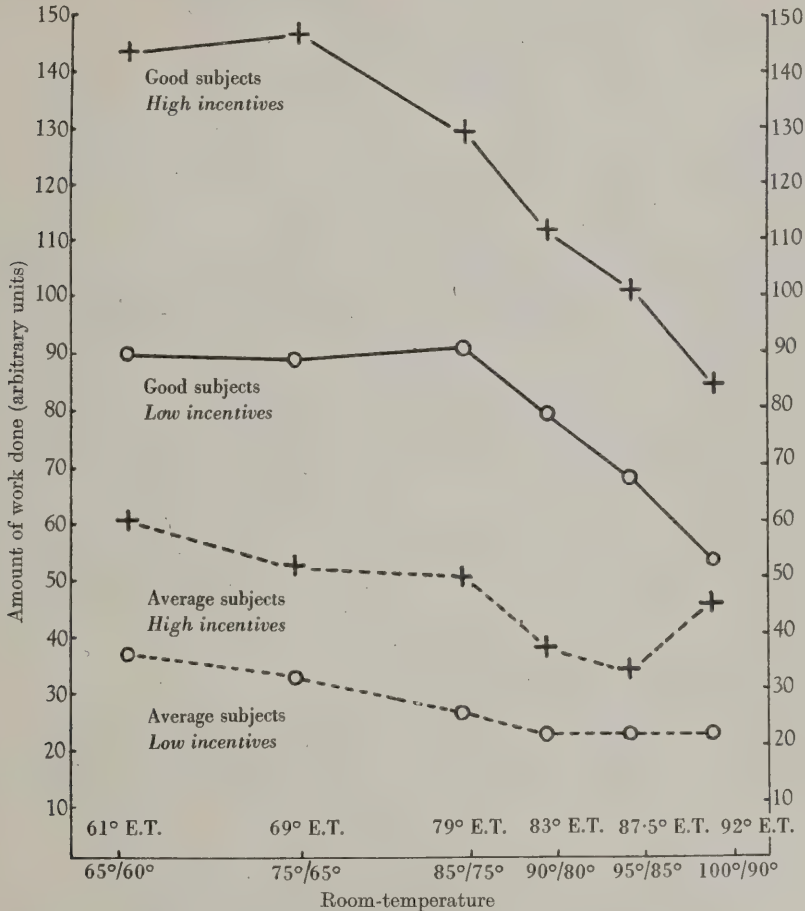


Fig. 4. Level of ability and the effects of stronger incentives in hot and humid atmospheres.

obtained in effective temperatures ranging between 61° and 79° F. and for that in the high effective temperatures of 83–92° F. The *t* value obtained on comparing the incentive increase for good and for average subjects in the relatively normal physical environment was 2.89 and at the high room-temperatures it was 2.49. This meant odds of more than 100 to 1 and 50 to 1 respectively against these being chance findings. It seemed reasonable to conclude therefore that in these circumstances high ability did not obviate the need for high incentives—in fact, on the contrary, if total output was the sole criterion of success, it was even more important to have high incentives for subjects of superior ability than for those of only average competence.

An attempt was then made to discover whether the good subjects and the average subjects deteriorated the same amount in their output scores when exposed to high atmospheric temperatures. Table 7 compares the absolute readings at the various room-temperatures.

Table 7. *Average amount of work in arbitrary absolute units*

Level of ability	Room-temperature					
	65/60° F. (61° E.T.)	75/65° F. (69° E.T.)	85/75° F. (79° E.T.)	90/80° F. (83° E.T.)	95/85° F. (87.5° E.T.)	100/90° F. (92° E.T.)
<i>Good subjects</i> (deterioration from 61° E.T. score)	116.3	117.6 (+ 1.3)	110.2 (- 6.1)	95.7 (- 20.6)	84.5 (- 31.8)	68.9 (- 47.4)
<i>Average subjects</i> (deterioration from 61° E.T. score)	48.4	42.3 (- 6.1)	38.0 (- 10.4)	29.4 (- 19.0)	27.9 (- 20.5)	34.1 (- 14.3)

Fig. 4 also suggests that the men with the higher initial output deteriorated more than those of average ability and this finding was therefore subjected to a statistical examination. For each subject the difference was calculated between his average output at the two lowest (normal) room-temperatures and his average score at the two highest room-temperatures. Since this reading was available once from the high incentive conditions and once from the low incentive conditions, there were thirty such readings for the *good group*, the mean for which was 40.2 units, representing a fall in output from 116.9 units at the normal temperatures to 76.7 units at the two highest temperatures. But the *average group* on the other hand gave a mean fall of only 14.4 units, from 45.4 to 31.0 units. Since this showed that the deterioration of the good men was nearly three times that of the average subjects, it was not surprising that statistical confirmation was forthcoming. In fact, a *t* value of 2.56 for the comparison showed that the chances were greater than 50 to 1 against this difference between the two average decrements being due merely to experimental error.

As before, however, the proportional deterioration was very similar in the two groups, a drop of about one-third in the output being experienced in both instances, i.e. in the *good subjects* the output at the two highest room-temperatures was only 66% of the score at the normal room-temperature, and this figure was 68% for the *average subjects*.

(5) *Comment on previous findings with different levels of ability*

At first sight this evidence appears to contradict previous work by the writer (Mackworth, 1946*a*) where it was shown in studies of wireless telegraphy reception errors that deterioration was less marked among men of exceptional ability than with operators of average efficiency, i.e. with an error criterion, the better the men, the less their deterioration—but here it has been found that, with an output criterion, the better the men, the greater their deterioration.

The key to this possibly rather false paradox is thought to lie in the fact that the highly skilled accurate performer requires less effort satisfactorily to complete his task of skill. (Some evidence in support of this contention is to be found in expert wireless telegraphy operators who can take part in light conversation while simultaneously writing down their messages without mistakes.) Therefore, in one respect, the men who make a low average error score on the wireless telegraphy test in normal room-temperatures

are like the men who under similar circumstances have a low average output on the pull test, in that they have not had to draw on their energy reserves. But men who have to struggle to keep down their errors on a skilled task even in normal room-temperatures, or men who are already working hard at some test of physical effort, have mobilized their stores of energy. Such men will therefore experience more difficulty in carrying the extra burden of abnormally high room-temperatures and will therefore show a larger rise in errors or a more marked fall in output.

IV. SUMMARY AND CONCLUSIONS

1. Powerful incentives failed to prevent the deterioration in physical output obtained when men were working to complete exhaustion on an arm ergograph apparatus in high atmospheric temperatures. Similarly, the introduction of strong incentives did not alter the critical level on the room-temperature scale above which performance began to deteriorate. As in previous investigations (Mackworth, 1946*b*) this critical level lay between 90/80° and 95/85° F. on the dry/wet bulb scale with an air movement of 100 ft./min., i.e. when the 'normal output' was measured at 85/75° F. (When the base-line for the task was taken at 65/60° F., the critical level was only a few degrees lower and lay between 85/75° and 90/80° F., since the latter figure was the lowest temperature to give deterioration.)

2. Knowledge of results, the provision of a target figure adjusted to the previous best performance of the individual and direct personal exhortation did, however, greatly raise the total output achieved on the pull test. In fact a 58% increase in the amount of work done was noted under these circumstances. (It is important to emphasize that these were spurt conditions and Crawley (1926) has shown that marked improvement on one such burst of activity may be detrimental to further physical work undertaken a few minutes later.)

3. Level of initial ability on the pull test was a definite factor in determining the extent of the incentive increase in output. These extra incentives gave twice the incentive increase with the good subjects as they did with those of average ability—and this advantage was present at high as well as at normal room-temperatures.

4. But the incentive increase in output was larger when the atmospheric temperature was normal than when it was unusually high.

5. Consequently the high incentive conditions showed a greater falling-off in absolute output with raised room-temperatures than did the low incentive conditions. (It was the greater scatter in the former readings which prevented the high incentive conditions from lowering the critical level.) But the proportional loss of output was the same in both instances, and was about one-third of the output at the normal room-temperature.

6. Similarly, the good subjects showed a greater absolute deterioration in performance in the high room-temperatures than did those of average ability. As before, however, the proportional loss of output was the same in both groups, and this was again about one-third of the output at the normal room-temperature.

7. The extensive utilization of reserves of energy (either by those working hard at a physical effort test or by men unsuccessfully straining to deal with a skilled task) is a predisposing factor to deterioration in hot and humid atmospheres.

Much general advice and help has been received from Prof. F. C. Bartlett, Mr E. G. Chambers and Mr G. C. Grindley of this Laboratory. I am particularly indebted to Dr J. N. Mills of the Physiological Laboratory, Cambridge, for measuring the energy requirements of the pull test. Thanks are also due to Mrs Nixon de Weber for preparing the illustrations for this report and to S. B. P. O. Edwards for clerical assistance. The experimental subjects were supplied by the Royal Navy—and the investigation has been undertaken for the Medical Research Council as part of a series of hot-room studies for the Royal Naval Personnel Research Committee.

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(*Manuscript received 1 August 1947*)

PUBLICATIONS RECENTLY RECEIVED

The Physical Background of Perception. By E. D. ADRIAN. Oxford: Clarendon Press. 1947. Pp. 95. 10s.

When, all too rarely, some worthy occasion successfully tempts Prof. Adrian to pause for a moment in his researches and bring together his findings, we have learnt to expect a rare combination of fascinating matter and beautiful clarity of style. In inviting him to deliver the first set of Waynflete Lectures, the Master and Fellows of Magdalen have placed many people, especially psychologists, in their debt, for this little book forms another instalment of the story begun in *The Basis of Sensation*. It will assuredly achieve the same classic status, and evoke the same affection.

The last ten years have been rich in experimental results bearing upon the physiological processes underlying perception, and Prof. Adrian outlines the advances made. With some assurance it is now possible to move inwards a step from the sense-organs to their functional and structural representation in the cerebral cortex, and a relatively large portion of the book contains an account of what is known of the immediate cortical consequences of external stimulation. Especially fascinating is the linking, largely through his own experimental work, of the behavioural propensities of various animal species with the amount of cortical area devoted to appropriate motor and sensory organs. Thus "the pig's snout is its chief executive as well as its chief tactile organ. . .", and accordingly a relatively huge cortical field is devoted to this part of its anatomy. In the chapter on the electrical activity of the brain, Adrian sets in order, and assesses the significance of, the various findings in this notoriously misleading field. In the last chapter he ventures into some provisional discussion of recognition and mental events generally, chiefly upon the basis of the work of Lashley and of Craik. One cannot help regretting that here Prof. Adrian did not allow us a larger glimpse of his own thoughts upon these topics.

It is all, as Adrian is careful to insist, of the nature of an interim report of a journey whose end the traveller is in no danger of reaching yet awhile. We may perhaps hope for another instalment later on.

Psychiatric Interviews with Children. By HELEN LELAND WITMER. New York: The Commonwealth Fund (London: Geoffrey Cumberlege). 1946. Pp. vii + 443. 25s. 6d. net.

It would be extremely interesting and valuable to the psychologist, as well as to the psychiatrist, to be presented with a systematic study of the methods of diagnosis and treatment employed by Child Guidance clinics, illustrated by detailed case histories. Unfortunately this book, though possibly its case material may be interesting to the psychiatrist, is too diffuse and unsystematic to convey very much to the psychologist. After an introduction dealing briefly with the nature of Child Guidance in the U.S.A. and the rôles played by doctor and patient at the clinic, ten cases are presented, by eight different doctors, of children whose ages range from five to seventeen, and whose disorders vary in severity. These cases, however, are not considered to constitute a representative sample of Child Guidance work. Different methods of diagnosis and treatment are described, but no examples are given of 'deep' therapy or of real play therapy. In each case, it is said, the patient could distinguish between reality and his own feelings; there was no interpretation to the patient of unconscious mechanisms; and only present conflicts were worked upon, with little attempt to search for their early origin. Thus little scientific study is possible of the origin of children's neurotic disorders, and little contribution is made to an understanding of the basic mechanisms and development of the child's mind.

1946 *Handbook of Neurology, Psychiatry and Neurosurgery.* Edited by H. Ruse, Mabel G. Masten, Nolan D. C. Lewis, and P. Bailey. The Year Book Publishers, Chicago. (English Agent, H. K. Lewis, 136 Gower Street, London.) Pp. 732. \$3.75.

The three sections of this review of experimental and clinical contributions made during the period under survey deal with Neurology, Psychiatry and Neurosurgery. The general plan follows that of the other publications in the same series. It is an attempt to select a sufficiently complete abstract for the student who already has a fair amount of knowledge of such fields to keep himself reasonably up to date. Each section is prefaced by a brief editorial introduction which more or less indicates the main developments

which have characterized the period under consideration, and also contains a list of important publications. There is no doubt that the present rapid rate of progress, or at least of publication, in neurological and psychiatric medicine renders some regular abstracting or summarizing service absolutely essential. And there is also no doubt that, subject to the fact that the selections made are not likely wholly to satisfy anybody, the abstracts published in this issue of the Year Book are very well done indeed.

A Way of Life for the Handicapped Child. By EIRENE COLLIS. London: Faber & Faber. 1947. Pp. 183. 10s. 6d.

In his foreword to this book Dr Somerville Hastings, who has had abundant opportunities of seeing the author at work, pays her a decisive tribute. To deal successfully with the child suffering from any form of cerebral palsy, he says, "the spirit and outlook of a pioneer is needed—keenness, vision, infinite patience and determination to succeed in spite of every difficulty. Of all these virtues Mrs Collis is possessed, and in addition of a capacity to profit from the work of others and learn from experience." The evidence of the book is that Dr Hastings is right on all counts. He might also have added that Mrs Collis can write clearly, concisely, from a wealth of knowledge, objectively, and that she can be honest without being scarifying. The volume should be of vast value not only to worried relatives, but also to anybody who is concerned with the treatment and training of children with cerebral palsy. Also it raises a number of important general problems in the field of the learning and control of specific movements which any experimental psychologist will find of great value and interest. It is, in fact, an outstandingly good book.

Psychology in Action. By JOSEPH CLAWSON. New York: The Macmillan Company (London: Macmillan & Co.). 1946. Pp. xii + 289. 18s.

It is almost certain that no country other than America would or could have produced this book. Both the author and the publisher have gone as far as possible to make it popular, or at least look popular. In style it is as dramatic as the writer can manage to make it, with snappy sentences and paragraphs, and considerable partiality for slang. It is illustrated by ingenious diagrams, pictorial statistics, and by many reproductions of posters and advertising pictures which have achieved wide popularity. It contains a lot of good stories. It is persistently dogmatic. The claims made for it are not modest: "First the book consists primarily of thumbnail case studies embodying what is known about and what has been done with human psychology so far. . . . Second the cases are arranged according to the principles they have in common, and these common elements are unified in a new theory of psychology. . . . Third it is pointed out how to recombine the elements and apply them to daily problems. . . . Fourth an elastic practical formula unites these principles of behaviour in a complete, interlocking system of psychology."

In spite of all this, and in spite also of the fact that so far as ideas go the book is nowhere near as original or as well founded as is claimed, the volume is undeniably attractive and there is a good deal to be said for it. The key to the whole thing is the contention that American society is dominated by value judgements or reactions ("Our way of life consists of our values"), and that the relative importance of these can be reliably determined by the frequency of their occurrence in popular advertisement and other forms of propaganda. Thus physical values are asserted to have got the upper hand in the American way of life, with economic and social values a good way behind in the second place, and intellectual, political, religious and 'general' values lagging far in the rear. But the co-ordinating principles, or what are called the 'highlights' of value, for all the claims that the author makes, turn out to be very ancient and respectable. All values are utilized for pain-avoidance and pleasure-seeking, and, though much is said, nothing much that is new emerges, except, possibly, the assertion that all the values are changeable except the physical; and for this not over-much evidence is given. The value of the book, in fact, does not lie in its ideas, but in the tremendous gusto and conscious unconventionality of their exposition. Incontestably the author is wide-awake, and it will be a good thing if he equally arouses lots of readers everywhere.

The Psychology of Personnel. By HENRY BEAUMONT. London: Longmans Green & Co. 1947. Pp. xiii + 306. 10s. 6d.

Within certain limits this, a reprint of an earlier 1945 issue, is an excellent volume either for the student who is beginning his studies in this field or for the general reader. Its assigned purpose is to serve not as a text-book but "as a general introduction to the contribution which psychology has made and should continue to make with ever-increasing success to the solution of the problems of personnel management".

Prof. Beaumont who is Director of the Bureau of Industrial Psychology in the University of Kentucky has written a serious and commonsense volume following rather routine lines. The great bulk of the work referred to is American and not all can be regarded as immediately applicable in this country. In a good many places (instances are in the sections on noise and on fatigue) rather over-dogmatic statements are made which it would not be easy to substantiate in any exact way. Also there is little evidence that the author has gained much from a study of applied work in related fields, particularly from recent war-time experimentation. Nevertheless the book is certainly a good one for straightforward reading, and although it may be criticized in regard to a number of details, it presents a clear, attractive and just general account of personnel work.

Psychology Applied to Personnel. By HENRY BEAUMONT. London: Longmans Green & Co. 1947. Pp. viii+167. 10s. 6d.

This is really a kind of junior investigator's exercise book to accompany the same author's *The Psychology of Personnel*. Part I deals with Personnel Statistics and consists of a number of brief expositions of the statistical methods most commonly needed in personnel research, and of exercises which are intended to be carried out with the aid of a Basic Data table provided. Part II takes the chapters in *The Psychology of Personnel* one by one, and gives selected and annotated references for further study of the topics raised, and problems in which the principles expounded and the evidence adduced may be applied. Teachers and students using the general expository text will certainly find the book of applications useful. But it seems rather an expensive style of publication.

Graphology. By HENRY RAND. Cambridge, Mass.: Sci. Art Publishers. 1947. Pp. 200. \$2.75.

A detailed exposition is given in this book of the various characteristics of hand-writing which must be noted by the graphologist, and of the various character traits which they indicate. The former are illustrated in the text. The interpretation of any one piece of hand-writing must, it is said, be carried out as a whole, the characteristics being assessed in relation to one another. Moreover, to become an expert graphologist, it is necessary to use 'empathy' and feel oneself into the gestures of the hand-writing, and hence the impulses and emotions, of the original writer.

Some of Mr Rand's interpretations seem ingenious and convincing; others, such as those of the moral qualities, singularly far-fetched. Since this is intended as a handbook for students, he himself gives no validity data. But an introduction by Prof. A. A. Roback implies that the expert graphologist may produce character assessments which would be at least as accurate as those obtained from other projection tests. It would, however, be interesting to know if the various graphological experts agree in their interpretations of the various characteristics of hand-writing.

The Answer is . . . Your Nerves. By ARNOLD S. JACKSON. Kingswood, Surrey: The World's Work Ltd. 1947. Pp. vi+162. 8s. 6d.

This book was written by an American surgeon mainly to reassure people who are suffering from the effects of too much hurry and worry, unsuitable food and too much smoking, and so on, that the disorders they experience are not due necessarily to any organic disease. Provided the book does not encourage the neglect of symptoms of genuine disease or neurosis, it may be helpful in giving such a reassurance. It has, of course, nothing to do with nerves.

